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Psychology
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PSYCHOLOGY THROUGH EXPERIMENT

J. A. DEUTSCH, *Stanford University, U.S.A.*
N. F. DIXON, *London University*
BRIAN FOSS, *London University*
R. L. GREGORY, *Cambridge University*
IAN M. L. HUNTER, *Keele University*
HARRY KAY, *Sheffield University*
J. SZAFRAN, *Exeter University*
ALAN WATSON, *Cambridge University*

Edited, with Introductions,
by GEORGE HUMPHREY

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PREFACE

This book has two purposes, of which the primary one is of course technical. It is intended as a companion volume for a practical class doing the experiments, and its purpose here is to help train young psychologists to think experimentally.

The second purpose, certainly more ambitious, is to induce those primarily interested in the humanities to take a glimpse at the kind of thing going on in practical psychology classes. The editor can personally testify that in spite of the many protestations he has heard on both sides of the Atlantic from humanists and administrators there are many people in responsible positions who still have serious misgivings about the psychological laboratory. ('How *can* you experiment on the Human Mind?' And again: 'Surely experiments on animals must always be humanly irrelevant'.) This attitude is more common perhaps among the older generation and on the eastern side of the Atlantic. The general point is elaborated later. It is hoped that some at least of these disbelievers may be induced to read the introductions and perhaps even to glance through some of the experiments.

There are then two introductions; one for humanists, one for psychologists, no mutual exclusion being implied since none exists.

May the best men read them!

G. H.

INTRODUCTION FOR HUMANISTS

GEORGE HUMPHREY

Surprisingly often the question is asked why psychologists do experiments. On both sides of the Atlantic it is the humanist who seems especially disturbed by our experimental activities. The laboratory, the animal house and the slide rule are often faintly distasteful to him and he feels that the last sanctuary of the human spirit is being invaded.*

Two cases. Two of many current cases will illustrate the point. A philosopher of deservedly international reputation asked not long ago why psychologists bothered any more about instinct, when 'All you have to do is to look in McDougall's book [meaning his remarkable *Social Psychology*, first published in 1908], where you will find it all done up nicely. He's settled the question once and for all.' It was indeed done up nicely – too nicely, admirers of the experimentalist Tinbergen's *The Study of Instinct* (1951) will think. Then again there is the revulsion some people feel against the experiments on animals. The head of an important North American University was lately looking for a man to direct his department of Psychology. 'I want a human psychologist,' he said, 'not a rat runner.' Animal experiments will be discussed later; but it may be said here that they are as necessary for the psychologist as for the physiologist, and for the same reasons, and that the nature of the inquiry generally demands that free-moving animals be used.

These examples, chosen from many, illustrate the misunderstanding found among many humanists of what psychologists do in the laboratory and why they do it. Contributors to this book have been asked to bear the non-professional reader in mind; such readers will of course not be interested in the *minutiae* of classroom work, but it is hoped that this and the other Introductions, the statements of the problems and the results and applications of the experiments described – perhaps also the controls introduced – may give the non-professional readers a rough idea of the nature and purpose of the psychological experiment.

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* With appropriate modifications, this Introduction is the same as that in *Social Psychology Through Experiment* (Methuen 1962).

I. WHY EXPERIMENT AT ALL?

To experiment is, roughly, to check one's opinion of Nature against Nature herself. The foundation of much humanistic education lies in books; perhaps for this reason humanists in all ages, as a class, have been against experiment as an educational method. It seems to many highly cultured people to consist of 'fiddling with bunsen burners and turning knobs', not to involve 'real thought', and to be a sorry substitute for 'the study of the great minds'. But it will be seen that in point of fact books have often done great harm by crystallizing and ultimately perverting opinion, and that far from concerning itself with trivialities, scientific training and thinking follow a uniquely difficult path of intellectual humility. This intense kind of work necessarily comes into good experimental teaching, just as the benefits of 'the classics' issue from their own kind of teaching when it is well done – but only then.

Before dealing with the particular case of psychology the general problem will first be considered. Why experiment at all? Artists go out to sketch and often use human models. In much of their training, also, human models are employed. Scientists go to nature in the same way – physical scientists to inanimate nature, physiologists and psychologists to living creatures. Carlyle once wrote: 'The place where we are to get knowledge, even theoretic knowledge, is in the Books themselves.' This is valiantly spoken; as a universal and unqualified rule however it is just not true. Newman tells us, for example, that in Athens at its heyday there were no bookshops at all. Of course the scientist has to go to books, but finding out what people have written is only a part of it. He is interested rather in what they have done. For *reading is a secondhand way of holding the mirror to nature*. As Einstein has said: 'If you want to find out anything from the theoretical physicists about the methods they use I advise you to stick closely to one principle: don't listen to their words, fix your attention to their deeds.' (Quoted by Toulmin 1953, p. 16.) And in the fifth century of this era Proclus,* the Neoplatonist

* Courtesy of Mr C. E. Stevens, of Magdalen College, Oxford.

philosopher, tells of a man who found a lizard showing spots that were not according to Aristotle. His conclusion was that the lizard was wrong. When again Galileo di Galilei's astronomical reasoning and observation seemed not to conform with what was written, a sermon was preached against him on the text: 'Ye men of Galilee why stand ye gazing up into heaven?', though it is fair to say that Galileo received an apology from the head of the order involved. His contemporary Scheiner who claimed priority over him for the discovery of sun-spots was told, on communicating what he had seen: 'I have read Aristotle's writings from end to end many times, and I can assure you I have nowhere found anything similar to what you describe. Go, my son, and tranquillize yourself; be assured that what you take for spots on the sun are the faults of your glasses, or of your eyes.' (Fahie 1903, p. 130.) So far can books tyrannize over facts, and so far must Carlyle be allowed to be wrong. On the other side of the scale one may see an undergraduate, being shown something he has read about, not uncommonly exclaim with surprise that 'it really works'. What surprises him is that what the books predict really happens *to him*, so to speak. Before, he had 'book knowledge'; now he has 'personal knowledge', and the exclamation is the mark of the transition from one to the other.

Humanists against experiment. It is probable that the humanist has fought shy of experiment in all ages and for much the same reasons, though these are perhaps not always explicitly stated or even realized. Teaching men to control men has seemed a higher endeavour than teaching them to work with their hands. Skill in the use of military arms has always been an exception. The reason the ancient Greeks made little progress in experimental science is said to be that they felt manual work was somehow degrading for a free man. The Athenian man thought it comic and mildly unworthy that people should interest themselves in commonplace things such as the buzz of a mosquito (Aristophanes' *Clouds*), though in view of the alleged prevalence of malaria in the ancient world a little knowledge of this insect would not have been a dangerous thing. A trace of the same attitude seems to linger somewhere in this still persistent attitude of the humanist. Experiment is often disturbing or brings results disturbing to people of fixed – and irreproachably honest – opinions. To those who have never practised it, and therefore do not see it in its context, it does seem trivial, fiddling and somehow not worthy of the attention of the best minds and the best men, who ought, it is felt, to be occupied better than by peering into test-tubes and in general playing peeping-Tom into trivialities. Not so long ago, and in the presence of the writer, a philosopher laughed uproariously when told that a student had obtained a doctorate with a thesis describing in new detail the processes at work when an earthworm moves, an activity of great interest to biologists and one which forms a paradigm to important human functions. But wisdom is justified of all her children. Experiment often leads to material progress, which itself *is* often disturbing. Classroom experiment is indeed trivial and fiddling and often accompanied at school by tomfoolery; but not more so than learning the basis of any technique, from irregular verbs to five-finger exercises, and – that old example – the multiplication table, whether one actually learns the table or talks about matches in a matchbox. And the fiddling with test-tubes, the turning of knobs and other such manual activities, together with the laborious computing of probabilities, are of course in well-designed laboratory work as well as in research an insignificant-seeming though necessary part of the total experimental activity, which may call for an intellectual power, a concentration and a patience at any corresponding stage as great as that which any humanist may practise. Would anybody look down on Shakespeare because he once spent a large part of his time fiddling with a goose feather? The parallel only seems trifling because we take the technique of writing so much for granted, forgetting the infinitely laborious preoccupation with trivialities by which we all had to learn it, or even that these small movements, acquired with or without tears, are trivialities only when considered out of their context. The time will come when those responsible for the teaching of the succeeding generation will regard learning to experiment as being equally important with learning to read and write, and will realize that being taught to light a bunsen burner or to read the level of the liquid in a graduated tube is as fundamental as learning any of the basic skills of the primary classroom of today. It was indeed not so long ago in the history of our race that to possess these everyday skills of our society was regarded as unworthy of a noble man, or even, farther back, of a free one.

Bronowski. It is the same kind of thinking. In a recent discussion of the relation of science and society, Professor Bronowski made the point that the historian and the experimentalist use the same kind of reasoning process, and that as far as their thinking goes there is little difference between the two, or between the humanist and the scientist in general. Of course he is right. Human beings use the same basic mental processes whatever problems they are engaged on. But the experimentalist has this great advantage that he is able to check his conclusions by the *repeated appearance* of nature, not merely by the analogy (Bronowski) of other human beings. This direct comparison of hypothesis with fact is an advantage the historian would be glad to borrow. 'What Song the Syrens sang' we are told he can only conjecture; and however close his personal knowledge of songs and singers, he would be glad enough to check up on his thinking, to try the experiment and tape-record the performance.

The battle is not yet won. It will be said at this point that experiment and the teaching of experiment in general have long been accepted by humanists and that what has been said is shouting after the war is over. It is true that the Royal Society was founded over three hundred years ago, for the purpose among other things of practising 'the new experimental philosophy'; and that its activities and those of its members have won respect in Great Britain and everywhere else; further that largely under its leadership and that of similar organizations throughout the world the growth of experimental science during the last three centuries has everywhere been enormous. But some may be surprised to hear that in 1869 Helmholtz, who died only six years before this century began, was refused the chair of Physics at Oxford, now officially so liberal in these matters. According to Cyril Darlington, 'an agreeable young ... mathematician ... with a considerable estate in Lincolnshire' was instead appointed by the five electors, of whom three were scientists 'certainly not interested in experiment'. . . . The new professor 'lived to a great age and for just fifty years [i.e. until after the First War] he was successful in forbidding all new physical experiments in the Clarendon Laboratory'. Helmholtz on the other hand 'proved to be one of the greatest influences in the development of science, and his influence stretched beyond his own country and beyond his own time'. (1958, quoting *Nature* 1921.) *It is simply not true that the humanist – speaking generally – has accepted experiment on completely* *equal terms with his own kind of study*. Though the story is probably fabled of the classical headmaster who, when his chemistry specialist was taken ill just before an examination had to be set, remarked that he must 'take a day off and get up the subject'. But it *is* true that many humanists find it hard to believe that experiment demands real thinking.

Jargon. Finally, to anticipate, psychologists with other scientists have been reproached in high quarters for using 'jargon'. (It is in fact not so long since they were reproached for the opposite fault, of 'giving bad reasons for what everybody knows already'.) It is hardly necessary to point out that for precision of thought new terms may have to be invented. The writer would also remind humanists that when he was being taught to understand Vergil it was considered necessary for him to know and use the terms asyndeton, aposiopesis, hemistich, zeugma, tmesis and chiasma.

Why think? Try the experiment. The great Jenner, discoverer of vaccination, is often quoted. He is said to have asked the question at the beginning of this paragraph; but of course he did not mean that experiment and thought are mutually exclusive. The way he made his great discovery shows this. He first observed that milkmaids who had cowpox did not contract smallpox, and then had the thought that other people also might be protected by infecting them with the less serious disease. That is, he formed a hypothesis by a process of thinking. A hypothesis may be called an expendable opinion. But instead of continuing to speculate he immediately tested the hypothesis by trying the experiment. It is well for mankind that he did.

The scientist and the model. Lord Kelvin used to say that he could not think properly about any physical event until he had visualized a mechanical model of it. The term was extended by another great physicist, Mach, Kelvin's contemporary, who said that 'the atomic theory . . . is a mathematical *model* for facilitating the mental reproduction of facts' (Mach's italics; see his *Principles of Mechanics*, Open Court, Chicago 1893, p. 492). The implication intended by Mach was that such terms cannot cover the whole of nature but only certain relations in it that can profitably be described, ideally in mathematical terms. The term has spread until it is now used to describe a mental, often mathematical, construction, derived from observation, with which certain aspects of a natural event conform, and which can be used to suggest further experiment. Thus what used to be called the association theory in psychology would now be said to serve as an elementary model for mental events; one idea revives a second one which has been related to it in one of several specified ways. This statement was thought to be derived from experience; it was used in fact as the basis of many experiments; in an extended form it was used by the conditioned reflex workers for further experiments, and was put into mathematical terms by American workers and others. But it does only describe certain aspects of the events with which it deals; one of Pavlov's dogs secreted 13½ drops of saliva at the sound of a metronome previously associated with food. The *major* work gives time and date, but not the colour of the dog, the length of its tail, the height of the experimenter or the colour of his eyes. The book quite properly abstracted from all these things as 'irrelevant'. Every natural event is greater than description can ever be; abstraction must always be made, as the artist and the scientist both know. And of course, this does not mean that the models of science (or the pictures of the artist) can be *deduced* from natural events by the ordinary processes of logic. Einstein has pointed this out (Toulmin 1958, p. 43). Genius knows what to abstract *from*.*

The scientist and the way of humility. The way of experiment is on all sides the way of humility, of the man who knows he can never know all of reality, but can only deal with a small part of it; who checks his hypotheses, *wherever he obtains them*, against nature herself; whose opinions, to repeat, are expendable. A good experimentalist cannot be a 'know-all', for he knows too many difficulties. If he has been well taught he sees problems where others see dogmas. To know what must be accepted and what may be questioned involves intellectual activity of the highest order. 'He knows nothing and thinks that he knows,' said Socrates. 'I am better off, for I neither know nor think that I know.' The path of humility as taught by Socrates may serve as a motto for all entrusted with the task of teaching, whether primary or university teaching, experimental or humanistic. It is a humbler way than that of the man 'prepared to stand by his opinions', wherever acquired, from whatever authority and however apparently ethical in character, or boasts, as some do, that he knows 'everything that has been written' on this or that. Properly carried out it requires generosity of spirit and tough, disillusioning thinking. It is a fair rule that the better the scientist, the humbler the man – at least about his subject.

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* In the special theory of relativity abstraction was from the observer's point of view.

Newton wrote: 'I do not know what I may appear to the world, but to myself I seem to have been only a boy playing on the sea shore and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before me.' Of him, in the year of his death, Pope wrote: 'God said *Let Newton be* and all was light.' The quotations are well remembered separately, and generally forgotten together.

This then is a plea for experiment in general both as involving an intellectual activity of the highest power and excitement, and as a pre-eminently searching method of education.

Experimental thinking often makes dangerous thoughts. A final word: especially in view of the growing social importance of Science it is today of transcendent urgency for the future of mankind that the coming generation shall be taught how thus to interrogate Nature, human and otherwise. If axioms, apparently self-evident, have to be questioned, and they must be, everybody must know why and how.* It is at its peril that mankind will listen without questioning to the voice of that philosopher who has told us that whatever is clear and distinct is true. What could be more clear and distinct than that the sun moves every day across a stable sky? Or that Nature abhors a vacuum, when one can hear and see Nature abhorring a vacuum every time water is drawn in a pump? In spite of the air of slightly discreditable nonconformity and even of immorality that such inquiries often involve, such questions must be asked and people must be taught to ask them, or at least how they are asked. Few of course can learn to experiment like a Nobel prizeman; but even fewer learn to write like Shakespeare, in spite of the millions of hours still being spent on the drudgery of handwriting and literary composition.

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* The philosopher Wittgenstein apparently thought it somewhat discreditable that the axioms of psychology need examining. Certainly those of physics, chemistry and astronomy have been most thoroughly reformulated *as the result of observation and experiment* directed by reflective thought. 'I have all along proceeded *experimentally* . . .' said the astronomer Herschel in a letter to Sir Joseph Banks about 1782. (Original italics.)

When any branch of knowledge can experiment it must experiment or it will die a dreary death.

What of Psychology in particular?

II. WHY EXPERIMENT IN PSYCHOLOGY?

The answer to the question, why experiment in psychology, might properly be said to lie in the pages following these introductions, as well as in the preceding ones. But it is well first to consider some of the objections actually raised. The case against is presented more or less as the present writer has authentically heard it expressed at various times, by people all of whom he admires and respects. It is important to remember that to a psychologist some of it may now seem dated.

It is essentially impossible to experiment on mind. There is first the argument from the general nature of mind, an argument deriving ultimately, perhaps, from the philosophers. Mind is the ultimate observing tool, and therefore cannot be used to observe itself, any more, as a rather stupid undergraduate once said, than a telescope can be used to observe itself. Mind is private to its possessor, and the events that occur in it can only be communicated at a kind of second hand, through language and other intermediaries. Experiments involving such events are not verifiable, so that an essential advantage and characteristic of experiment, not hitherto mentioned, is lost. Another important characteristic of fruitful experiment, namely prediction, is also lost, since what cannot be verified cannot, *a fortiori*, carry over to a new context. Since, again, mind is the tool by which mind is observed, observation will alter that which is being observed. He who hesitates is lost, and the bank clerk of past days adding the columns of a passbook, while at the same time observing what is going on in his experience while he adds, would certainly be thrown off his balance, if the expression may be allowed. It is interesting to note here that the physical scientist has encountered this last difficulty, in that observation of his ultimate data has been found to interfere with the data themselves. Measurement of position changes velocity, and vice versa. Psychology – introspective psychology at that – may claim to have anticipated physical science in this point, and to have devised its own measures against the difficulty; though the physicist has left psychologists far behind by the elegant procedure of making virtue out of necessity, transforming uncertainty into the Principle of Uncertainty.

Further objections against experiment in Psychology. To add to the tale: mind and mental events, it is said, cannot be measured. As it has been said of pleasure, they admit of the qualifications *less* and *more*, but not of quantitative assignment. Thus the chief instrument of experiment is lost. Psychology has followed too closely the special (and often transient) theories of the philosophers, such as that built up by Hume on 'the best scientific models known by hearsay to him, namely those of mechanics and hydraulics' (Ryle 1953). Investigators have thus, it is said, been diverted into paths that lead nowhere, and for that reason experimental psychology has remained unproductive (Ryle, ibid.). It has, again, become known as the study which gives bad reasons for what is already known. Finally the very concept of an experimental psychology, whether of experience or action, assumes a Determinism which is against common sense, philosophical conclusion and moral decency alike.

It is a formidable indictment, and it comes mainly from philosophical sources. Of course, it must hastily be added that not all philosophers raise all or indeed any of these objections! Many who do raise them are not philosophers. But it may fairly be said that these arguments are all being currently advanced and by people with the same humanistic leanings as you may meet among experimental psychologists themselves. In summary: *Mind cannot be exactly observed or measured: for that reason experiments performed on it are*, in the jargon of today, *bogus experiments concerning already known trivialities. This is shown by their negligible practical results. The problems investigated have in fact been pseudo-problems. Experimental psychology must assume Determinism. It remains nugatory and pretentious, and minds of the highest competence keep away from it.*

The Behaviouristic revolution. These statements will certainly generate heat among most psychologists, since it will be said that the Behaviourist movement rejected subjective psychology for many of these reasons. There is a good deal of truth in this. Indeed the polemic of J. B. Watson (1914, ch. 1) against the subjectivism of the early twentieth century is and will long remain a classic; it said many of these things with a finality that has never been equalled. Well known though the argument is to psychologists, it is apparently still not familiar to many in other disciplines.

The gist of the argument was that the 'introspective psychology' of the time was unproductive and by its nature could not be scientific. By introspection was meant the study of one's own conscious experiences, 'thoughts', to use a popular term, as contrasted with activities. It is clear that everybody can observe the latter, while only the thinker can directly observe his own thoughts. (The conventional argument is here given.) The data obtained by introspection in this sense depend on the 'say-so' of the person being investigated (or, it turned out, on the 'say-so' of the particular psychological grandee under whom one was working).

The question arose, for example, whether mental 'Images' were essential for the process of thought. It should be pointed out that there are many kinds of mental images, such as visualized pictures of past or distant events, imaginary sounds as of an orchestra, imaginary movements of the muscles, and so on. The great German psychologist Wundt, whom the writer had the privilege of hearing, claimed that images were necessary, and so did many of his pupils. The almost equally eminent Anglo-American Titchener and his pupils said they were. The brilliant Külpe, together with his pupils *en bloc*, said they were not; but that the process of thinking was entirely independent of any process of forming mental pictures of any kind. Who could decide between these eminent men? Each supported his conclusion by experiment. Another instance, this one given by Watson. Clearness was said to be an important attribute of attention. It would be said that when for instance I attend to the sound of the lawn-mower outside, the noise becomes clearer, while other things become less clear in attention. Some psychologists maintained that there were nine different degrees of clearness, others gave a different number. Still others denied that 'clearness' had anything to do with it. Who was to decide between them? The writer once heard the great Titchener himself (the author was a very young and admiring man) being rotated on a revolving chair, and describing his conscious experience. It made a long and detailed story, quite different from that of another well-known psychologist who came round later. *Both were eminent men*. Who could judge between them? Perhaps they were both right. *Who knows?* Nobody but Titchener could verify Titchener's story. Such subjective facts, i.e. depending on the conscious experience of one person only, cannot form the basis of a science, said Watson. We must have data that can be observed and verified by any qualified person, as in every other science. In the case of psychology these data are not difficult to find. They are comprised not of 'conscious experience' but of 'behaviour', what the organism 'does', not what he 'thinks'. Whether a man blinks his eyes, whether he jumps from his chair, moves the muscles of his face when he is learning to write, this can be observed and if necessary registered, e.g. photographically, for everybody to see. If there are 'individual differences', a phrase much in fashion just before that time, these are verifiable. Henceforward again, Watson claimed, psychology is to concern itself with Behaviour, what an organism does, what everybody properly trained can see and verify, neglecting altogether what goes on 'in the mind', as unverifiable, unrepeatable and not amenable to scientific treatment.

It was a promising, valiant programme, and psychology has never been quite the same since. It did seem to give an answer for the many objections to 'subjective' experiment and to introspective psychology that were already growing up. This introduction will show later that what it did was really to redefine mind so as to make it susceptible to conventional experiment, and this will appear to be an important step forward.

It is fair to say that Behaviourism (Behaviorism) in its rigorous and original form, which in fact proposed to solve the psychological problem by liquidating its difficult aspects, has itself proved impracticable; and that the specific hypotheses and 'gimmicks' such as calling introspection 'verbal behaviour' which were designed to waterproof the leaky spots in the programme have now been substantially dropped. Few if any modern psychologists profess a rigorous behaviourism, few will deny the energy that Watson's original programme brought with it.

Really Behaviourism *per se* seems in the end to have solved no problem at all, but rather to have changed the psychological coordinates and perhaps also to have shifted the point of experimental attack. For instance, the sensation, 'the characteristic element of perception', was to go, and its place was to be taken by the reflex; but the reflex itself is now recognized to be a 'convenient abstraction' only, a phrase which might very probably be found in the heated discussions concerning the sensation at the beginning of the century. Association, again, involved the nebulously subjective 'ideas'. Its place was to be taken by the conditioned reflex. But this in its turn bred its own special difficulties, and so on. Further, the important Gestalt school, now more or less merged into the body of psychological doctrine, dealt at least on its polemical side almost entirely with subjective data, and attempts to transfer it to the behaviouristic side of the fence have never been completely successful. The behaviouristic dogma is now honoured as much in the breach as in the observance, and psychologists, particularly experimental ones, get their information when and where they can. Except for a little information from electrical recordings, mental images for example must be treated as 'subjective' data. Except again for a few approaches by way of electrophysiology the study of sensory processes calls for introspective report and so on. Behaviourism has really resulted in a widening of the field, not in its complete translation; many of the old problems are still with us but we call them by different names. It is of course true that nobody now practises the elaborate introspection of the early twentieth century laboratories, and the science is certainly the better for it. But to defend experimental psychology by crying 'we have changed all that' would be to take refuge in a revolution that has never really taken place; there has been a change of government only – perhaps one might say a coalition with a minority.

The widening scope of psychology. In fact instead of throwing out 'subjective' events, as Watson tried to do, the modern worker has, rather, enlarged the scope of his investigations until this now includes the activities of the whole organism, *acting as a whole*. These activities lie on the side both of experience and of behaviour. When he sees those who follow other disciplines interesting themselves in things that seem to lie on his side of the fence, he does not worry. Who cares who does the work as long as it is well done? An eminent Oxford philosopher recently asked what was happening at the Psychological laboratory. When told, he exclaimed in a shocked voice: 'But that is surely physiology!' It concerned the *perception* of colour during short temporal exposures. Was it physiology or psychology? The work might equally have been done in either laboratory. But it is of course the humanist, if anybody, who has been insisting that the psychologist ought to confine his interest to 'subjective' events, such as perception, and would be disinclined to let the physiologist trespass on this field.

Back to Aristotle. It is important here not to allow oneself to be seduced by easy etymology. If anybody still insists that psychology must concern itself only with the *psyche*, meaning the soul or the mind, the modern psychologist may properly refer him to Aristotle, who said: 'If the eye were an animal, vision would be its *psyche*.' It is the total functioning of an 'organic body' (Aristotle) that the experimentalist is now content to examine. He includes under this rubric events both of behaviour and of experience; and he prefers human beings as the subject of his study, when he can get what he wants from them. Sometimes he cannot, when he has to use animals. When he does use human beings, experience has taught him how to avoid the faulty experimental situations concerning which so much has been made, and he tries to work round to what he wants through the observation of behaviour or otherwise. Recognizing, for example, that an event in experience cannot be measured in the same way as a sheet of paper, Fechner with Weber found that a variable factor in the corresponding *situation* is *related to the experience in a definite way*. Then by 'reading off' the measurement of the physical factor, which he can get, he obtained what serves as a measure for the corresponding experience. The same thing is done when a man reads the height of the liquid in a thermometer. The height of the mercury is not the temperature of the room, but is related to it in a definite, relatively simple way and may therefore be used as a measure of it. Fechner's 'measurement' of sensation, so bitterly attacked by philosophers and psychologists alike, is no more and no less legitimate than that of any scientist taking any pointer reading.

It is desirable here to draw together the threads of the argument even though running the risk of too much summarizing. Defining mind in terms of conscious experience, it is claimed that the idea of (mental) observation of mental data is impossible, if only for the reason that to observe a mental event changes it. All such events, being 'private', are incommunicable, unverifiable, and not amenable to prediction. 'Mind' is incompatible with measurement. These points may be grouped under the term 'Arguments from subjectivity'; Watson recognized their cogency, and in effect changed the definition of Mind (as the subject matter of psychology) by throwing out all 'subjective' factors. His attempted revolution has proved to be too drastic, and psychologists have reverted to something close to the Aristotelian concept, now studying the total functioning of the whole organism, as contrasted with the science of physiology, which tends to interest itself in part-functions.

To return now to the fundamental question: *Why experiment in psychology?* The answer is now clear: because in no other way can psychological progress be made. Here as elsewhere it is not possible to rest on the dictum that what is clear and distinct is necessarily true. To the early psychologists, for example, it seemed obvious that the intelligent person was quicker in the uptake, more accurate, had a better memory, could discriminate better, and so on. Corresponding 'mental tests' were therefore given to school children and even to undergraduates. Unfortunately and surprisingly they were found to bear little relation to intelligence. Interest waned, as often, till the arrival of a genius, Binet, who changed the whole method of attack. In essence his method consisted of finding out what the average child, at successive ages, can do – that is, the whole, functioning child (Aristotle would have been delighted) – and in this way he built a measuring instrument. If a child can do 'eleven-year-old things' (e.g. in a very early list, detect the nonsense in absurd statements, find three rhymes to a word, etc.) then mentally he is thereby functioning at an eleven-year level. His 'mental age' is eleven, at least. To make such age-levels ceaseless experiment was necessary, and many *a priori* assumptions had to be thrown overboard. 'It will be necessary to experiment for a long time,' said Binet, the master (he was speaking of the special characteristics of the mental defective; his work on tests was done 1905–11).

Like everything else then, psychology has its 'clear and distinct' truths, which must be tested by experiment in order to discover a firm basis on which to build a science. Here, as elsewhere, *no other method* can give such firm results or such real understanding to a student and psychologists are fortunate in that methods *have* actually been found by which they have been enabled to experiment in their subject.*

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* It must be remembered, as Professor Mace points out, that psychology owes an enormous debt to people like Galton and Lorenz, neither

Practical results. It is easy to see that experimental psychology must be a subject of extreme difficulty, as every subject is at first. But working within this flexible framework, and on a strictly experimental basis, great and intensely practical things have been achieved. In one hour a competent sergeant major with a few corporals can now do what would have taken the great captains of the past months of bloody experience. He can roughly sort a thousand recruits into those who can and those who cannot learn what is necessary for an officer to know; mistakes are, of course, made – who has not made them? A textile mill can be shown how to cut a week off the training of its operatives. Millions of hours of children's time have been saved as they learned to read. Thousands of old men and old women are in the process of being shown what work they can do, and do it as well as or better than their children. Thousands of aeroplane pilots have had their training time reduced, their instrument display improved, their ability to read the numerical and other scales of their instrument-panel enormously bettered, and the whole of the complex system of controls and other material conditions of flying fitted to the man-at-work. Under the old system the man was expected to adapt himself to the machine. A psychologist can now say when a locomotive driver is liable to over-run his signal, and that under conditions of extreme fatigue he may just not be conscious of a light that is 'staring him in the face'; psychologists have in fact done this. To judge a tree from its fruits is today both unfruitful and unscientific; but these and many similar things, all experimentally achieved, are not the product of an endeavour that is self-negating (the mind cannot observe itself!), incompatible with precise measurement, incapable of prediction, platitudinous, trifling, incapable of verification, essentially self-interfering, and in general the product of second-rate minds. Detailed refutation of all these charges is here surely unnecessary.

Freud. Mention should specifically be made of the man who 'has made the greatest contribution to psychology since Aristotle' (McDougall). Many people equate modern psychology with psycho-

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of whose methods could be called strictly experimental. It may be wondered whether psychology – which like other experimental sciences has grown out of natural history – requires more 'naturalists'.

analysis, and warning should be made against this. Strictly speaking and from the purist's point of view it might be said that Freud was not a psychologist at all. That is to say, he was not one whose chief interest lies in the understanding of the subject and is reasonably conversant with the knowledge and the techniques that have been developed by others so interested. He was a psychiatrist, that is, one whose main interest is in healing. But he was a uniquely gifted psychiatrist, one who finding that contemporary experiment was not adequate for his purpose had the genius to make a system of his own for normal psychology. He did not believe in psychological experiment as it then was – nor do we of today – and his own system was therefore non-experimental. But it did enable him to understand both normal people and his patients. It contained what now seem to experimentalists to be absurdities; of course, for it had not itself been subject to experimental test. It succeeded in shocking public opinion, thus putting its maker into the same category with many other geniuses. For one reason or other many experimentalists find Freud's writings exasperating, but everybody must admire the strength, the lucidity and the ingenuity of the mind behind them.

Freud and Fechner have both contributed greatly to psychology from without the subject. Would that the future may bring many more such!

Wittgenstein. Before ending this introduction mention is due of Wittgenstein, the great philosopher who has probably had more influence on philosophic thinking than any other for a generation. What he says, at the end of an important book (1958), probably crystallizes the thoughts of many philosophers and other humanists. 'The confusion and barrenness of psychology is not to be explained by calling it a "young science"; its state is not comparable with that of physics, for instance, in its beginnings. . . . The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by.' This passage, which comes at the end of the important *Philosophical Investigations* (1958, p. 232 e), may fairly be taken as the gist of this philosopher's view on psychology. It must first be noted that the reference is here and throughout to a 'subjective' psychology, now, we have seen, discarded in favour of one closely proposed by a still greater philosopher. (This must be said in spite of the statement made and discarded in the same book that 'Psychology treats of Behaviour, not of the Mind', p. 179.) The confusion and barrenness of a purely introspective psychology have in themselves long ago been exposed – by psychologists themselves and long before this Introduction was written. Concerning the reference to physics, it is only possible to say that nothing can be imagined more barren than early physics with its four elements, earth, air, fire and water, or more confused than early eighteenth century physics, with its mythical heat-substance phlogiston, and that it was experiment which ultimately substituted more workable categories in each case. As for the reproach that the experimental method does not apply to psychological data, this charge has in fact already been refuted when it was shown that psychologists have shifted their point of attack in such a way that experiment is possible, real, productive and indeed necessary for psychological progress. One must come to one of two conclusions. Either Wittgenstein has read neither the accounts of experiments actually done nor their practical results, reference to some of which has been made; *or* by some verbal legerdemain he explained them away to himself as not being 'psychological'.

The influence of Wittgenstein on philosophy is great; the passage in question, and others which could be quoted, do however show once again that opposition to experimental psychology is still very much alive in serious and influential quarters. The whole procedure of much of the opposition is in fact lamentably easy; first state what in your opinion people in another discipline ought to be doing, or assume they are doing it; then, show that this work is nugatory – irrespective of what they actually *are* doing. On this charge Wittgenstein cannot be acquitted.

This justification of experimental psychology to humanists, and this regrettably brief answer to an important philosopher, may fittingly finish with a quotation from one who was himself an eminent humanist. It is perhaps somewhat distorted from its context, but it is still not amiss:

> *He that seeks after knowledge, let him fish for it where it may be found.* (Montaigne, *Essay on Books.*)

July 1958

REFERENCES

FAHIE, J. J. (1903). *Galileo: His Life and Work.* London.
RYLE, GILBERT (1953). *Acta. Psych.*, **9**, 189–96.
TOULMIN, STEPHEN (1953). *The Philosophy of Science.* London.
WATSON, J. B. (1914). *Behavior.* New York.
WITTGENSTEIN, L. (1958). *Philosophical Investigations* (tr: Anscombe). Oxford: Blackwell.

INTRODUCTION FOR PSYCHOLOGISTS

Although it is intended to be used with a class doing experimental work in psychology, neither contributors nor editor would wish this book to be regarded as a 'textbook' in the ordinary sense of the word. It is intended rather to show students, in practice, the kind of reasoning and procedure by which Psychology has made such startling progress in the last generation or so. The book may be used with classes at all levels, including those, for instance, whose main interest is in psychology, as well as others who do not wish to specialize in the subject but would like some knowledge of it for professional or other reasons. Examples of experiments suitable for non-specialists are Reaction Time and a simple form of the conditioned reflex.

There is no special reason why those who do a little psychology should do a different psychology, though they may well take longer to understand and perform a given experiment.

As regards intending specialists it was thought that by making his own personal selection and adding experiments of his own, the instructor would find at least a year's experimental work reasonably covered. In response to the wishes of contributors chapters are supplied with fairly full references. Interested pupils like them. The instructor's interpretation of any experiment will of course not always agree with the contributor's, and this has real teaching value.

Now is perhaps the place to point out that the book has been written to illustrate no particular 'system' or psychological slant, except that it does illustrate and exemplify the experimental method throughout. Eight good men, experimentalists, were asked to write chapters on their specialities, responsibility for the choice of contributors resting with the editor and one or two of the contributors.

The original endeavour was to provide experiments that could be performed by a whole class at once, so that the practical work might go in gear with a general lecture on psychology. Thus it was hoped to solve a problem faced by those whose fate it is to deliver such a lecture (Sir, Dr Jones spent an hour on that in the practical class last week!). In spite of such good intentions, however, it was found that certain experiments could not be arranged for simple enough apparatus, thus keeping, as well, within the financial resources of the average laboratory; though the editor was surprised at the ingenuity shown by contributors here and at other points.

The book keeps strictly to fundamentals and, it is hoped, will be followed by volumes dealing with more specialized subjects. The first of these, *Social Psychology Through Experiment* (ed. Humphrey and Argyle), has recently appeared with Messrs Methuen. Even with this restriction it was obviously impossible to cover the whole field, so that the book contains in effect sample experiments on sample topics. Thus, once more, room is left for individual instructors to air their favourite experiments or demonstrations. With a little ingenuity certain experiments may be done at home during the vacation, if one dare mention such a possibility! Some could not be made quantitative and these with certain others must therefore be considered as demonstrations only. All have been successfully used in class by psychologists who know their business, success of an experiment in practice being one of the criteria for inclusion. Here an indulgence must be asked for Dr Dixon, who stepped at the last moment into the breach with the chapter on 'Perception'. On behalf of everybody concerned with the book the editor would like to tend him special thanks for the remarkable speed with which he wrote his chapter. If as he wished he had taken time to try his last two experiments with a class, publication already long overdue would have been intolerably protracted. The reader will find, however, that the quality of these experiments well qualifies them to form a single exception to the rule. Some may find the chapter 'difficult'. It should be. Perception *is* difficult! *Psychology* is difficult! Here and in all other places criticism will be welcomed.

Besides the criterion that experiments must work (everybody at least of the writer's generation must remember class demonstrations or experiments in physics or chemistry that did not!) contributors were asked to provide exercises that would *train*, in the sense of teaching people to think psychologically and about psychology. They should also be taught to read the psychological literature, some of which is found difficult by even the best of beginners. They should

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be taught, with the instructor's help, to write up their own research. Psychological principles should thus be experimentally derived by the undergraduate, and the inherent problems brought out. It is hoped also that pupils may be encouraged to start further research of their own. All this adds up to a rather formidable task, and it should do so. For the way to this most difficult of all studies is not easy.

It means, in fact, and this must be said very humbly, that an instructional experiment should, ideally, be simple in procedure yet profound in application, technically easy as far as apparatus and other conditions go, quantitative, rather sternly described, workable, illustrative, instructive, undogmatic. At which every contributor to this book as well as every other psychologist will rise at once to join the writer in crying out that it cannot all be done always. Yet it *is* attempted, at least, in many first year classes, e.g. in university physiology and biology, not to speak of physics and chemistry, for which, however, there has probably been previous preparation. The editor feels that this volume is the better for the rather grim ideal.

It has already been indicated that contributors are people whose business it is to conduct practical classes; they have been chosen for their reputation both as psychologists and as teachers and have contributed according to their special fields. They form a not unworthy selection of the exciting new generation of psychologists now growing up in this and other countries. The policy has been to interfere as little as possible with what they have written, in order to preserve the freshness of individual style both of thought and of manner. Even such minor details have been left as the different abbreviations to be found in the literature the psychologist must learn to read. At risk of repetition it must again be emphasized that the book is intended to give a framework for the instructor to fill in, not a complete account of an experimental course. If it differs from the many experimental manuals now on the market this may lie in part in the fact that it is written by a group of research people primarily to help train research-minded people. The editor has never found that honest thought is shirked by students of any grade.

Contributors are thanked for the extreme patience they have all had to exercise, in all too many ways. He would also, on behalf of both contributors and himself, thank Mr Peter Wait, of Messrs

Methuen and Company, who must for a long time have felt weary of the whole project.

G. H.

July 1958–February 1962

Since there are items which experimental conditions demand should be unavailable to subjects before the experiment is done, *instructors only* may buy from the publishers at a cost of 2*s*. 6*d*. an envelope containing the necessary material. Without it and without this restriction the experiments of Chapters 3 and 6 are meaningless. Instructors using the book are asked to treat as a matter of professional courtesy the necessity of keeping this material permanently from the class. The nature of the subjects makes inevitable a difference of difficulty in the chapters – also, if one may say so without impertinence, the nature of the contributors!

It is important to notice that since for personal reasons the writer of Chapter 8 on Statistics was unable to complete his assignment any references to this subject should be understood as referring to:

HOEL, P. G., *Elementary Statistics* (1960). New York and London: John Wiley.

The editor is grateful to Professor R. C. Oldfield and Dr Michael Treisman for this suggestion. Both are of Oxford University. The book in question may be considered as an elementary statistical companion to the present volume.

The explosive growth of psychology has long overtaken publication-lag: certain recent developments could not be adequately treated.

Chapter 1

SENSORY PROCESSES

R. L. GREGORY

University of Cambridge

We may think of perception as the end result, the output, of physiological systems adapted to handle information originating from the environment. When we see a table, hear the sound of a chair dragged across the floor, feel the cold hardness of the wood or the softness of the cushion, highly complex mechanisms are involved. It might be said that as we come to understand these mechanisms we *explain* perception. There may be other kinds of explanation of perceptual phenomena, but certainly one important approach is to discover the sensory processes involved. This is no simple task.

To study sensory processes in man we make use of intact people who respond in some agreed manner to various stimuli, or signals provided by the experimenter. The data from an experiment will consist of the physical values of stimuli, such as the brightness, colour and area of spots of light, and the recorded responses of the subject. The subject is not expected to report on his own sensory processes, and yet his reports are used to discover what sensory processes are involved. How is this possible? The answer is that the experimenter has in mind some sort of theoretical model, or set of models, of what might be going on. Many psychological experiments on sensory processes are attempts to provide evidence for or against predictions which are made from hypothetical models of the system. Some experiments are, however, purely exploratory. Curiosity or practical need may suggest some situations as worthy of investigation.

Appropriate models may be suggested by thinking about instruments, or devices for handling information, or purely mathematical or statistical models may serve. When it became possible to measure the electrical activity in nerve fibres during controlled stimulus conditions (Adrian 1928, 1932) a new world was opened up. Although immensely important, it should be realized that only a minute part of the total activity can be detected and recorded at any one time. The physiologist does not escape the necessity of thinking in terms of models just because his data are taken from inside the system. He must decide whether a given effect is important or a mere artifact; and if it is important just what its significance is. This can only be done in terms of some conceptual model of the system.

Verbal reports have some advantages over the various physiological records we can obtain from animal preparations or from human subjects. In particular, when an investigator tries something out on himself he has a unique opportunity to note unexpected effects.

The theory of information, due to Shannon (1949), has opened up the possibility of obtaining quantitative measure where only a few years ago this was impossible. It has been clear for a very long time that the nervous system conveys information from the sense organs to the brain, via afferent fibres, and that the brain may command the muscles, via efferent nerves. These nerve fibres clearly carry *messages*. Adrian (1928) discovered the key to the physical form of the messages when he discovered that all nerves transmit trains of electrical impulses, the rate of these impulses increasing with stimulus intensity. Information theory enables us to say just how much information such systems could represent, or transmit, in a given time.

Science has so far primarily concerned itself with energy systems, but the problems of the control of energy – involving information – are now receiving detailed attention. The distinction between energy and information can be puzzling, for any information system requires energy in order to function. Energy is required to produce speech sounds, radio signals or nervous messages. Such systems cannot, however, be understood simply in terms of energy. The eye must have sufficient light energy to function, but the organization of this energy into temporal and spatial patterns is also vital, and requires information concepts for its understanding. It is the particular form of the activity of groups of nerve cells under various conditions of stimulation which is important. The organism must function as an efficient energy system to allow the processes to take place which give us thought and perception, but an important part of our understanding of these processes is not in terms of energy but rather in terms of the flow and handling of information.

THE STRUCTURE OF THE EYE

The eye is often compared with a camera. Like a camera it has a (nearly) light-tight box, the *sclerotic-coat* with its black *choroid lining*. The light enters the circular *pupil* through the transparent *cornea* which forms a bulge of about 8 mm radius, the radius of the eye-ball being about 12 mm. The size of the pupil is controlled by the *iris*, as in a camera lens. The lens of the eye lies behind the cornea, the space between them being filled with a lymph-like fluid which is renewed about once every four hours. 'Spots before the eyes' may

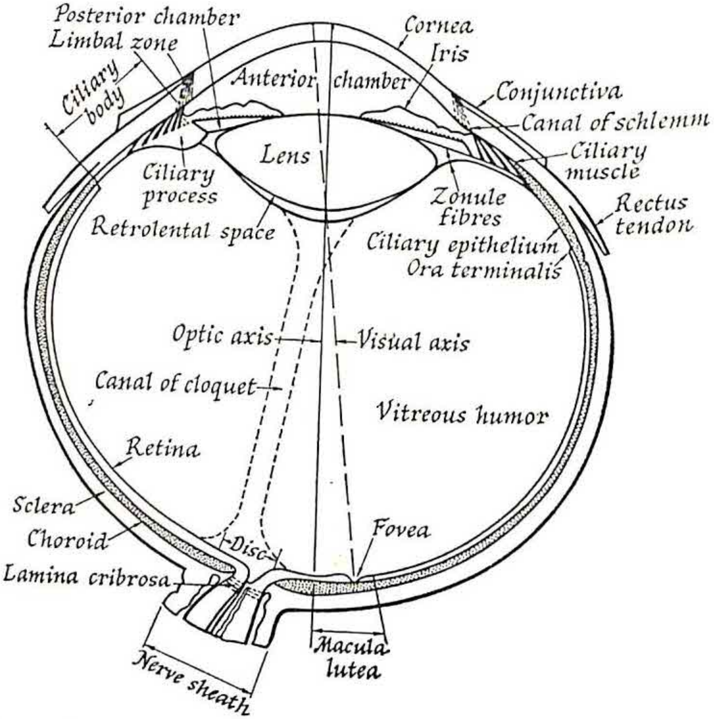

FIG. 1-1. Diagrammatic cross-section of the human eye. (From Walls 1942, after Salzmann.)

be caused by particles floating in this *aqueous humour*. The lens of the human eye is quite unlike a camera lens for it is flexible, and kept under tension. Its radius of curvature, and thus its focal length, is modified during *accommodation changes*, by the *ciliary body*.

The image is formed primarily by the air/cornea surface, for it is here that the light is bent most, since the difference between the refractive index of air and that of the aqueous humour is greater than between the aqueous or vitreous humour and the lens. A man who has had a diseased lens removed can see tolerably well. The fish does not have the advantage of a low refractive index in its external medium. Its lens is denser than that of man, and nearly spherical in shape. It is not flexible and moves bodily in the eye to achieve accommodation.

Examining the Purkinje images during accommodation changes. It is just possible to do this with no apparatus beyond a lighted candle; but a cocoa tin with an arrow pierced in its side, and containing a frosted electric bulb, is more suitable. If this is placed in a dark room and so arranged that the illuminated arrow may be seen reflected in the subject's eye, three images of the arrow can be seen, the images being produced by reflection from the convex surface of the cornea and by the front and back faces of the lens. By requesting the subject to accommodate to objects at various distances, the relative changes in size of the images may be noted. This is best done by viewing them through a small telescope clamped in a retort stand, but this is not essential. The following images should be noted:

The Purkinje images. With the eye accommodated to infinity ('emmetropic') the following images should be visible:

1st Image – right way up, bright – from the convex surface of cornea.

2nd Image – right way up, large and dim – from convex surface of lens.

3rd Image – inverted, small and bright – from the posterior surface of lens.

If the subject now accommodates to a near object only the *3rd image* (from the posterior surface of the lens) changes in size to any marked extent. It becomes smaller, approximating to the first image in size but inverted. This shows that in accommodation the radius of curvature of the posterior surface of the lens is reduced.

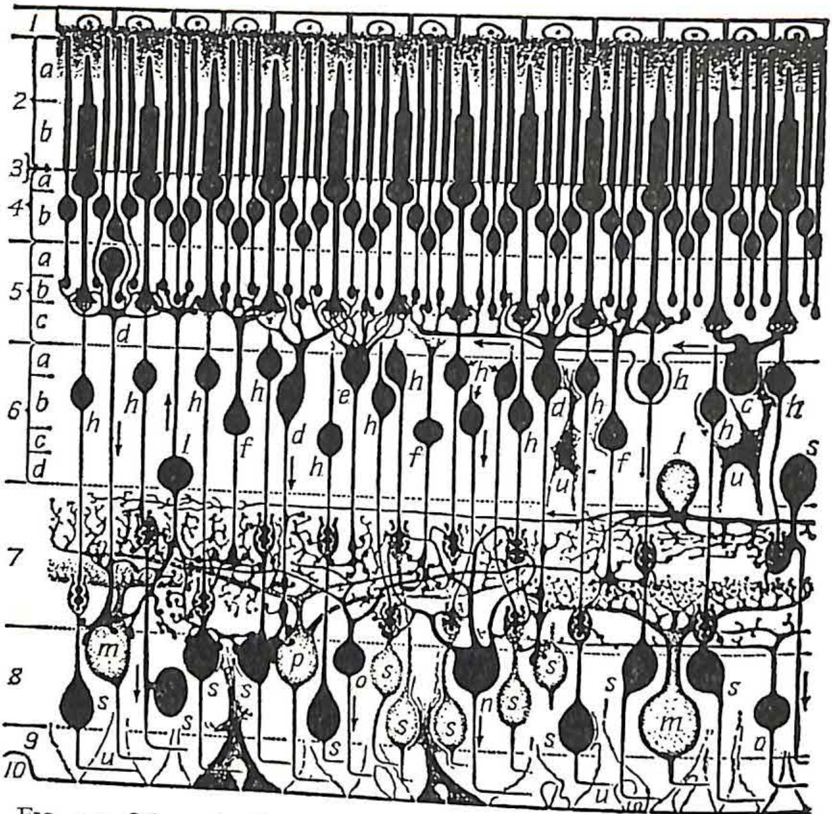

FIG. 1-2. Schematic diagram showing the neural microstructure of the retina of the monkey, based on Golgi impregnations. The layers are:

(1) Pigment epithelium
(2*a*) Outer segment of rods and cones
(2*b*) Inner segment of rods and cones
(3) Outer limiting membrane
(4) Outer nuclear layer
(5) Outer plexiform layer (cone-pedicles and rod spherules)
(6) Inner nuclear layer
(7) Inner plexiform layers
(8) Ganglion cells (origin of primary visual projections)
(9) Layer of optic nerve fibres
(10) Inner limiting membrane

c, horizontal cells; d, e, f, g, bipolar cells; i, l, amacrine cells; m, n, o, p, r, s, Ganglion cells.

(From S. Polyak, *The Retina*, Chicago, 1941)

The mechanism of accommodation was discovered by Helmholtz,* and is described clearly in Fulton (1955) pp. 430–3.

The retina. The light-sensitive layer, the *retina*, at the back of the eye has been described as 'an outgrowth of the brain'. It is a complicated arrangement of receptor cells (the 'rods' and the 'cones') and various other kinds of cells, some of which provide cross connections between the receptors. Both excitatory or inhibitory interactions may occur across the receptors, due to cross connection. The structure is described by Polyak (1941); evidence of interaction is derived from recording activity at the ganglion cells, Kuffler (1953), Barlow (1953), Hartline (1940, etc.).

The receptors. These are of at least two kinds: the *rods* and the *cones*. They are so named from their appearance, but Willmer (1946) has pointed out that in the central *foveal* region the cells are very tightly packed, and the 'cones' in the fovea look similar to the rods of the peripheral regions. Although the foveal receptor cells look like rods they are believed to be, functionally, cones.

The rod cells are highly sensitive, and provide 'scotopic' vision in dim light. They do not provide colour vision. The cones give 'photopic' colour vision, but require more light than do the rods. The spectral sensitivity curves of the rods and cones are different, as may be seen in Fig. 1-3. The change in the wavelength for peak sensitivity changes over a rather critical range of intensity. The change in wavelength of the peak sensitivity is known as the *Purkinje shift*.

Osterberg (1935) counted the rods and cones across the retina and found that the ratio of rods to cones increases from the centre to the periphery. In the fovea there are, probably, only cones and in the extreme periphery virtually only rods. There are, in a single retina, about 125,000,000 rods and 6,500,000 cones. There is a marked convergence of the receptors onto fibres, particularly for

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* Hermann Ludwig Ferdinand von Helmholtz (1821–94). As a physicist, contributed to the theory of the conservation of energy, as a physiologist to the so-called law of specific energies and to the determination of the rate of conduction of nerve activity. As a psychologist, to the study of vision and hearing, where his work is outstanding. The *Handbuch der physiologischen Optik* (first volume 1856, last 1866, tr: 1924) and the *Tonempfindungen* (1863, also translated: N.Y. Longmans, Green) are classics, and remain important works of reference.

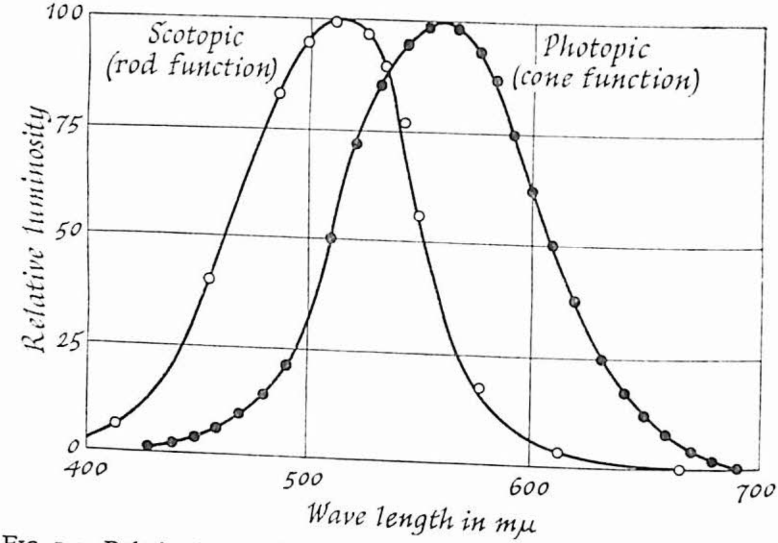


FIG. 1-3. Relative luminosity curves for human rod and cone vision. (After data by Gibson and Tyndall and scotopic data by Hecht and Williams.)

the rods. The foveal cones seem to have one–one connection with their fibres.

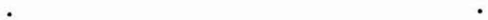
The sensitive receptor cells lie at the back of the retina, behind layers of nerve fibres, blood vessels and various supporting cells. The light has to pass through all these obstructions before it reaches the receptors, though the obstructions are minimal in the central *foveal* region. We do not normally see these obstructions, and at first sight this is surprising. The slightest speck of dust, or a fine hair, lying on a camera film will produce a clear shadow image. The reason we do not see such shadow images from the blood vessels is that light always reaches the retina from precisely the same direction – down the optical path. Consider a receptor lying behind an obstruction; it will always be in shadow, and it adapts so that its sensitivity is higher than neighbouring receptors free of obstruction. This *selective adaptation* is thought to prevent the fixed obstructions being visible. If it were possible to shift the direction of the light reaching the retina these obstructions should become visible. A beautiful and well-known demonstration makes this possible.

Demonstration to see one's own retinal blood vessels. The only apparatus required is a small torch. The naked bulb of an ophthalmoscope is ideal for this, as it gives a small source of adjustable brightness.

The experiment is best done entirely on one's self, or the eye may be hurt. First remain in a dark room for at least five minutes, then place the lighted bulb in close contact with the shut lid of the eye, near the temporal margin of the orbit. If now the bulb is moved with regular oscillation, a startling tree-like pattern will emerge and remain visible while the light is in motion. The pattern will die away rapidly when the movement is stopped – evidently because selective adaptation again occurs – but during the movement the shadow pattern will be displaced across the retina so that new receptors are affected. This never occurs in nature.

It is worth thinking out just why, normally, light always reaches the retina from the same direction, and noting also that this is why the pupil appears black. It is impossible to see into someone's eye because light would have to come precisely from the direction of the observer's eye to illuminate the part of the retina observed. The secret of the ophthalmoscope is that it sends in a beam of light on the path along which the observer looks.

The blind spot, where the optic nerve leaves the eye, is readily demonstrated. It may be 'seen' by fixating the left-hand dot with the right eye only (or the right-hand dot with the left eye), holding the page vertically,



at a distance of about 9 inches. The peripheral dot will disappear as it falls on the blind region.

If a homogeneous field, such as a sheet of white paper, is fixated we do not see a black patch. The visual system tends to extrapolate whatever is seen across the blind spot. This is a remarkable thing; and it warns us how 'high level' perceptual processes may complicate the simplest observation, and may confuse theoretical issues.

EYE MOVEMENTS

Eye movements were studied in detail by Dodge, early in this century. He found, using photographic techniques, two main types of eye movements, and these occurred under different conditions. When the eye is swept across the field it moves in a series of small jerks, called *saccades*. These occur about four times per second. Barlow (1952) says: 'There is general agreement that the most usual type of movement is a rapid jerk, followed by a period of ·1–·3 second during which the eye is relatively stationary, and terminated by another quick movement bringing the eye to a new fixation position. When the eye is following a moving object it moves smoothly, once it has (found) the object and is able to follow it.' These two types of movement seem to be quite distinct, and actuated by physically separate brain systems. Holmes (1938) finds that some patients with certain lesions literally cannot take their eyes off whatever is fixated, sometimes whether the object fixated is stationary or moving, without recourse to deliberate blinking, or some other such trick to break the visual contact.

We may demonstrate quite convincingly the *saccadic voluntary movements* and *smooth following movements* with only simple apparatus.

Demonstration of eye movements. A small neon lamp, of the type used as indicators in electronic equipment, is placed in an otherwise dark room and viewed from any convenient distance, of the order of 6–10 feet. The neon is arranged to flicker at a rate of 20–30 flashes/second. (This may be obtained by feeding it from an A.F. oscillator, by increasing the output voltage with a suitable step-up transformer.)

The flicker rate is adjusted so that when the neon is fixated its light appears continuous, but if the neon is moved steadily across the field it will be seen to flicker. The steady light is seen when the flicker rate is above what is called the *critical flicker fusion frequency* (c.f.f.) and this is lower for a fixed than for a moving image. (The value of c.f.f. is a function of the brightness, the area and the level of adaptation of the eye. It is of the order of 30 flashes/second.)

If the image moves smoothly across the retina the flicker is seen as a train of luminous dashes, and the limited recovery time of the retinal receptors does not limit the ability of the system to distinguish flicker because successive sets of receptors are brought into play with each flash.

If the eye is swept slowly across the field, flicker is not seen. The jerky movements of the eyes prevent the smooth sweep of the image which is necessary to see the flicker. If, now, the eye follows the movement of a small self-luminous object the regular flicker *is* seen, *provided the eye is accurately tracking the object.* This situation enables us to verify a new and rather surprising finding about what may be called the 'smooth eye movement' system.

Eye movements and proprioception. If the observer's hand is held before the eyes and tracked, although the hand is completely invisible in the dark, the neon being feeble, the flicker may be seen, indicating that smooth eye movements can be mediated by proprioceptor information. Evidently the tracking need not be visual for smooth eye movements to take place (Gregory, 1958).

It seems that the eyes move in saccadic jerks, except when following moving objects, in order to prevent images effectively fading as selective adaptation occurs.

THE PERCEPTION OF DEPTH

The images on the retina are on a curved surface, but it is not misleading to call them 'two-dimensional'. The visual world, on the other hand, is three-dimensional – we see things in depth. The processes giving depth perception are complicated, many being 'perceptual' in that we discuss them in terms of properties of the stimulus field rather than of the visual system. Such factors as apparent size, texture gradients and perspective will not be discussed in this chapter – we shall consider only some of the specific sensory processes involved in depth perception.

Demonstration that the use of both eyes is important for depth perception. This may be shown in many ways. *Hering's Fall* is a standard method; here small beads are dropped at various distances from a vertical wire placed about 1 foot from the subject. He reports whether the beads pass in front of or behind the wire. Another method is to place two (or sometimes three) vertical rods some distance from the subject and so screened that the top and bottom of the rods are hidden: they are thus seen in a horizontal slit perhaps a few inches in width. One of these rods is placed at various distances while judgements of 'in front' or 'behind' the comparison rods are made. The difference between one and two eyes is very great. This experiment might be done for various distances from the display, to show that the advantage of binocular vision falls off with distance.

It is worth obtaining not only estimates of the limits of discrimination of distance, but also some observations of perceptual changes experienced when occluding one eye. The following is very striking. Place an unmarked white china tea cup say 10 feet from the observer, so that he looks almost directly into it. The illumination should be even, and originating from behind the observer from a single source. There must be no shadows cast into the cup. If this is viewed with two eyes nothing odd will occur, but when viewed with one eye it will be seen to be a reversible visual situation, like the well known Necker cube, only here we are looking at a real object. It is an odd effect, difficult to describe but simple to set up and examine.

When a near object is viewed binocularly three factors should be considered. (1) The eyes will *converge* upon the object and, like a range finder (the 'base' between the eyes being about 2½ inches), the angle of convergence might provide information as to the distance of the object. (2) The retinal images will be *disparate*, that is, they will differ according to the two directions from which the object is viewed. These two views are evidently fused centrally to make one picture having depth. (3) Relative *displacement* of images on the retinas.

These factors may be isolated quite simply, and so investigated and assessed individually.

Convergence. (1) May be artificially increased or decreased with four mirrors* arranged as shown in Fig. 1-4 (*a*) and (*b*), or reversed, so that the right eye is effectively in the left socket and *vice versa* as in Fig. 1-4 (*c*).

(2) The light entering the eye may be bent. This may be done by interposing prisms having small angles (5–10 degrees) between the

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* Ordinary back-silvered looking-glass mirrors may be used. The mirrors may be mounted for ready adjustment on photographic tripod ball-and-socket heads. They may be attached to these by brass plates to which the heads are fitted with a tapped hole, and which are stuck to the back of the mirrors with 'Bostik' rubber glue. The plates are necessary to give sufficient area for the glue, but need not be larger than about 4 inches square. The mirrors can be positioned by simply pushing them, once the tripod heads are set to correct friction with the screw adjustment. This is a useful dodge, having many applications. The heads themselves can be mounted on a wooden or 'handy angle' frame.

object and the eye. This changes the effective distance of objects while retaining the same retinal disparity. Thus while disparity is held constant convergence and shift are modified.

Disparity can effectively be removed simply by viewing an object which gives the same image from any view point. Evenly lit spheres, or vertically oriented cylinders, may be used.

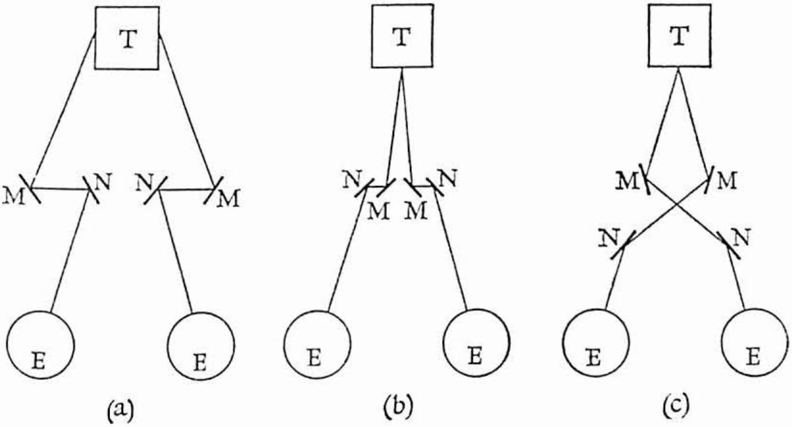

FIG. 1-4. (*a*) The telestereoscope. This increases the disparity.
(*b*) The iconoscope. This reduces the disparity.
(*c*) The mirror pseudoscope. This reverses the two eyes.

Shift may be demonstrated by holding the two forefingers before the eyes one in front of the other about 2 feet apart. When fixating the nearer finger the more distant finger will be seen double and vice versa.

The factors may be investigated in a stereoscope using suitable photographs or drawings. This has the advantage that *any* two figures may be presented to the eyes, which is particularly useful for considering the phenomena of *fusion* and *rivalry* of the retinal images.

DISCRIMINATION

A. Intensity discrimination. A distinction is often made between the *absolute* and the *differential* sensitivity of the eye. This is certainly convenient experimentally, for the stimulus conditions are different for the two cases, but it is not so clear how these conditions differ from 'the point of view' of the brain.

For determining an *absolute threshold*, the eye is stimulated with a spot of light viewed against complete darkness. The 'stimulus' or 'signal' may be of any size, colour, shape or duration, and in any position with respect to the eye.

For determining a *differential intensity threshold*, a *background* field is added, the subject being required to discriminate the signal field from the background. The absolute case always has an implicit background, however, for the light from the test field will scatter in the eye, stimulating surrounding receptors. Further, the retina is not entirely 'silent' even in complete darkness, as has been shown with electrophysiological recording by Kuffler, Fitzhugh and Barlow (1957) for the cat, and no doubt this is true also for the human eye although we have no such direct evidence. Random background activity, 'noise', is present in all sensitive communication systems, and tends to set a limit to sensitivity. This is probably true also for the senses, as was originally suggested by a television engineer, Rose (Gregory and Cane 1955; Barlow 1956, etc.).

1. Weber's Law for intensity. Consider a single candle in an otherwise dark room. If a second candle is now lighted, the room is perceptibly brighter, but if a candle is lighted in a room filled with daylight its added light, although the same as before, will not be perceptible. In the first case the illumination was increased by 100 per cent, but in the second the candle represents but a small percentage increase in the total illumination. Now Weber's Law states that the just detectable difference in intensity is a constant proportion of the background intensity. If the increase is ΔI and the background I, this may be written

$$\frac{\Delta I}{I} = \text{constant}$$

The constant is known as Weber's Constant, C, this being about one per cent though its value is different for each sense and depends on many factors such as adaptation of the sense organ and, in the case of vision, the area of test background fields.

Weber's Law does not hold true for small values of background intensity. When I is small a relatively greater change (ΔI) is required for detection. Weber's Law may be extended to cover very small values of I by adding a small constant to the denominator, thus:

$$\frac{\Delta I}{I + k} = c$$

The value of this hidden constant, *k*, can be determined graphically

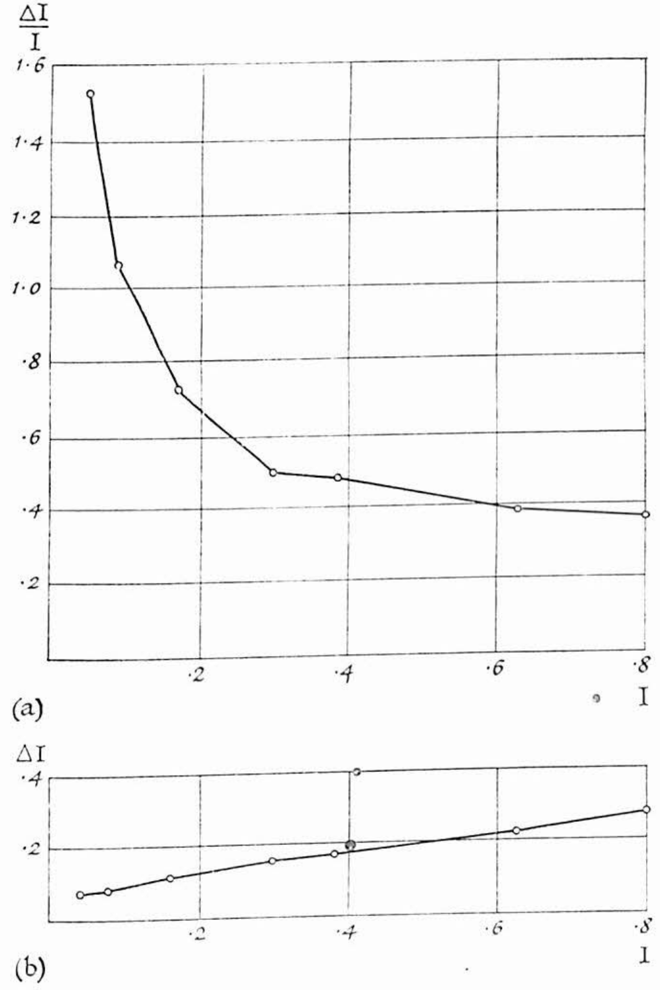

FIG. 1-5. (*a*) Shows experimental points for ΔI/I against I for low intensities where Weber's Law breaks down.

(*b*) Shows the same data as in (*a*), but plotted as ΔI against I.

by plotting ΔI against I and extrapolating back past the origin along the I-axis. The constant might originate from the 'dark light of the retina' or the random 'noise' background of the neural system.

The absolute threshold may be thought of as ΔI/*k*. This added effective light, 'noise', would become insignificant for all but low values of I, which would explain the breakdown of Weber's Law. Indeed, we may be rather more confident as to why the Law breaks down at low intensities than why it holds in the first place. We may say that constant proportional sensitivity is a useful characteristic of most measuring instruments, and so argue that Weber's Law is useful for the senses. A rather different suggestion has been made by Barlow (Barlow 1957) in discussing the idea of sensory discrimination as limited by the random noise level of the system. Barlow regards Weber's Law as a puzzling special case holding only for large areas.

He considers the case of small areas, and finds that a square root relation holds. Now this square root relation is attractive, because it would be expected if the eye were like a photocell discriminating 'signal' from random 'noise' in the cell. The whole question of the

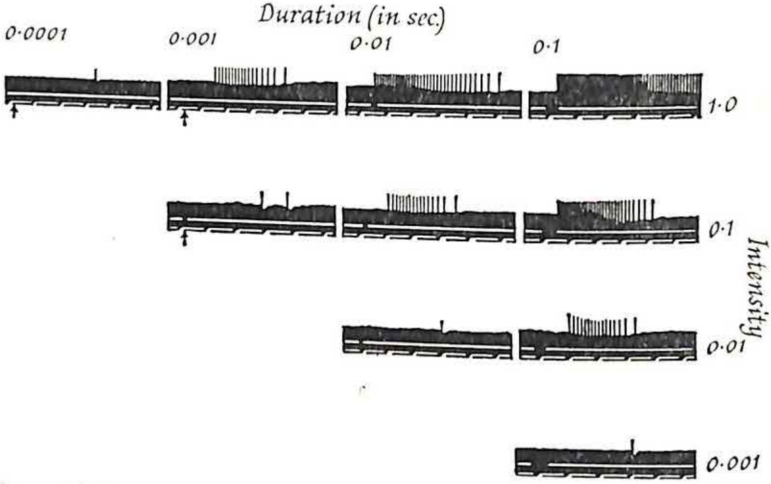

FIG. 1-6. Reciprocal relation between intensity and duration shown in terms of activity in a single optic nerve fibre of the horseshoe crab *Limulus* (Hartline 1934) The recorded activity in mammalian optic nerves is much more complicated because of the intervening retinal cells which are not present in the Limulus eye.

status of Weber's Law, and the conditions under which it holds, is still open to debate, although it has been studied continuously since the birth of experimental psychology.

2. *The effect of flash duration.* For either the absolute or differential threshold this is given by the Bunsen-Roscoe Law, which is:

$$I \times t = \text{constant (where I is the intensity and } t \text{ the duration)}$$

for durations less than about ·02 second.

3. *The effect of test field size.* For either absolute or differential this is given by *Piper's Law* which is:

$$I \times \sqrt{A} = \text{constant (where A is the area of the field)}$$

providing A is not greater than about 15–20 degrees or less than about 6 minutes of arc. For very small fields *Ricco's Law* holds. This is:

$$I \times A = \text{constant}$$

4. *The effect of position on the retina.* For the absolute threshold with dark adapted eye, the greatest sensitivity is 15–20 degrees from central vision. This is where the density of rods is greatest. Measures of absolute sensitivity are made in this region, and it is an old trick of astronomers and mariners, looking at faint objects, to use this region by 'looking off' the position of interest.

B. Spatial discrimination – acuity. The problem of what sets the minimum size limit below which we cannot see a gap in a line, a dot in a clear field, or the texture of a surface, is more complicated than the factors limiting intensity discrimination. Acuity is, however, probably limited for the normal young eye by intensity discrimination.

When we speak of the size of an object in this context we speak loosely: we mean the *angle subtended at the eye* by the test object. Acuity is expressed as the subtended angle, in minutes or seconds of arc (where 1 minute of arc = 1/60 degree, and 1 second of arc = 1/60 minute, or 1/3600 degree). In addition to giving the *subtended angle*, however, the *distance* of the observer from the object should always be stated, since his acuity may be affected by the amount of accommodation required to bring the test object into focus.

It is an important optical fact that images are never perfectly sharp. Consider the image of a point of light against a perfectly

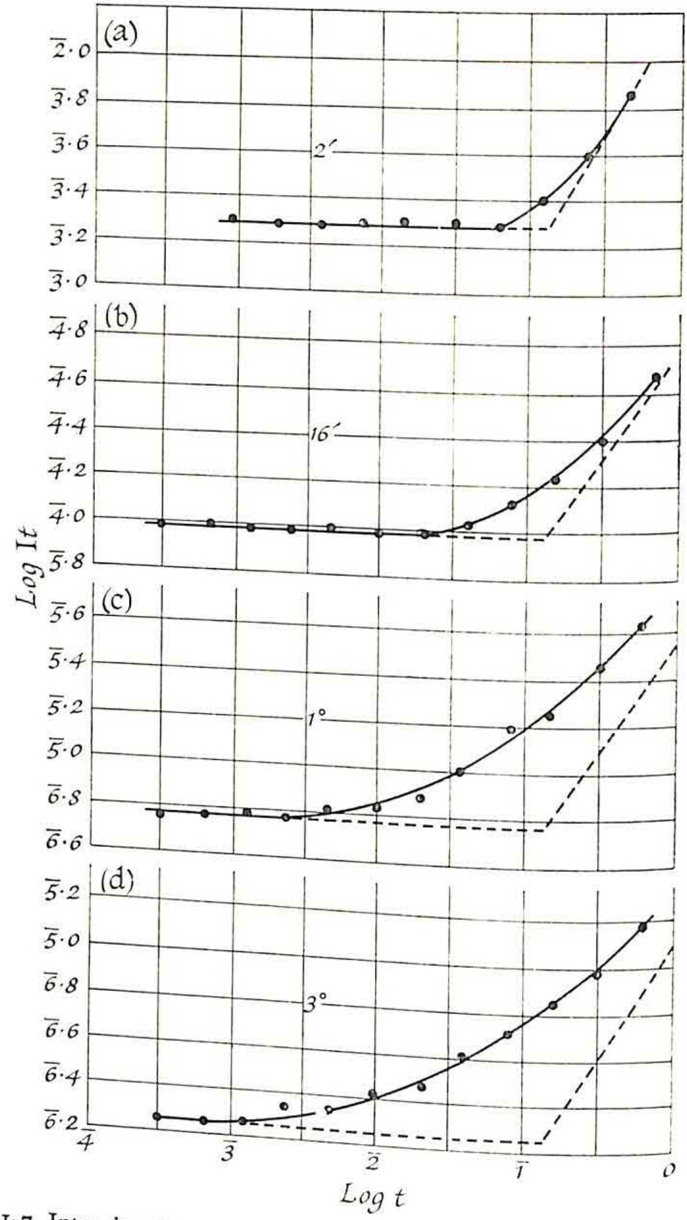

FIG. 1-7. Intensity–time curves for four areas for the human eye. For short durations the relation $I \times t =$ constant holds. It may be seen that the breakdown for longer times occurs earlier for large than for small areas. The reason for this is not known, but it is difficult to see how the explanation could be in terms of the photochemistry of the retina. (After Graham and Margaria 1935.) (Areas increased progressively from top to bottom.)

black background – say a single star on a black night. If this were a point source (which is true of a star), the image is never a point but is a disk having finite size. The size of the disk will increase with the brightness of the source. The finite size of this disk is due to a number of factors, in particular to aberrations of the lens, spread of light through and across the retina, and to diffraction.

The intensity gradients across the retinal image will in general be less steep than those of the object. Hecht and Mintz (1939) have considered the importance of this in acuity, and give the following diagram relating the spread of photic energy to the density of the cone receptors. (The scale at the top of the figure represents a mosaic of cones of 2·3 μ diameter, this being the average size of the central foveal cones.)

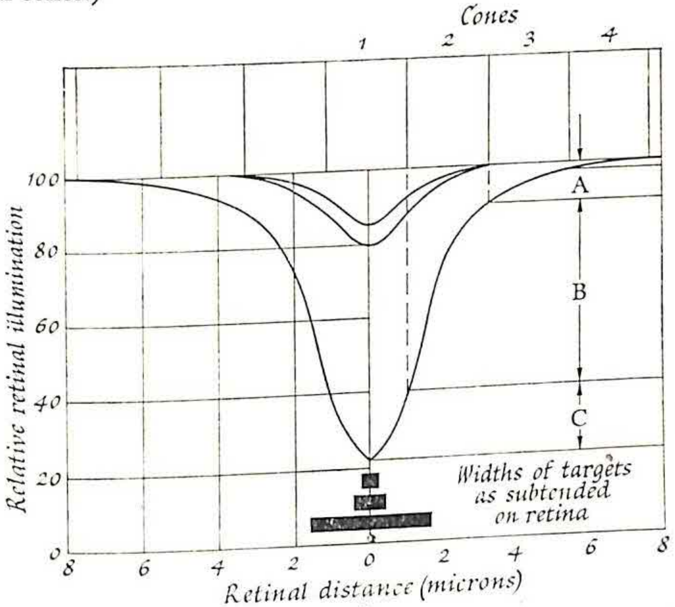

FIG. 1-8. Light intensity distribution in diffracted images produced by the fine wires whose geometrical images are shown as three lines in the lower part of the figure. The scale on top of the figure represents the retinal mosaic, consisting of cones whose diameters are 2·3 μ, this being the average size of foveal cones. (From Hecht and Mintz 1939.)

It is clear that the image will spread across several cones, in two dimensions of course. If we try to resolve a double star we have the following situation, for each star will produce its own diffraction pattern and these will overlap if the angle subtended by the stars is sufficiently small.

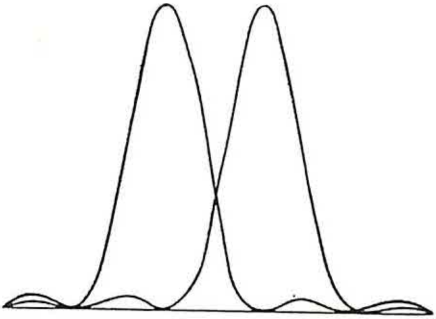

FIG. 1-9. Light intensity distribution from two points or wires. For discrimination the two peaks must be detected from the trough between them.

The problem confronting the visual system is to discern the fall in intensity between the two peaks. Thus acuity becomes ultimately a matter of intensity discrimination.

If this is so we should expect area to be important, for we have seen (Ricco's and Piper's Laws) that smaller intensity differences may be distinguished with larger than with small areas. This has been confirmed for acuity: Anderson and Weymouth (1923) showed that the visibility of a fine line increases with its length. We need not be surprised to learn that for fine lines acuity is so high that the geometrical image may be smaller than the diameter of a single cone: in fact the image may be but 1/60 of a cone diameter. Evidently the intensity gradient information may be summed across many receptors, each contributing some information.

Hecht and Mintz (1939) found that a long fine wire subtending ·5 second of arc could be distinguished against a bright homogeneous background. This is the extreme limit under optimal conditions; an average value might be about 1 minute of arc to resolve two points.

EXPERIMENTS ON ACUITY

1. *Acuity as a function of intensity*. Acuity increases with intensity as shown in Fig. 10.

Many attempts have been made to give some simple explanation of this curve. In particular, Hecht suggested that this is the integral

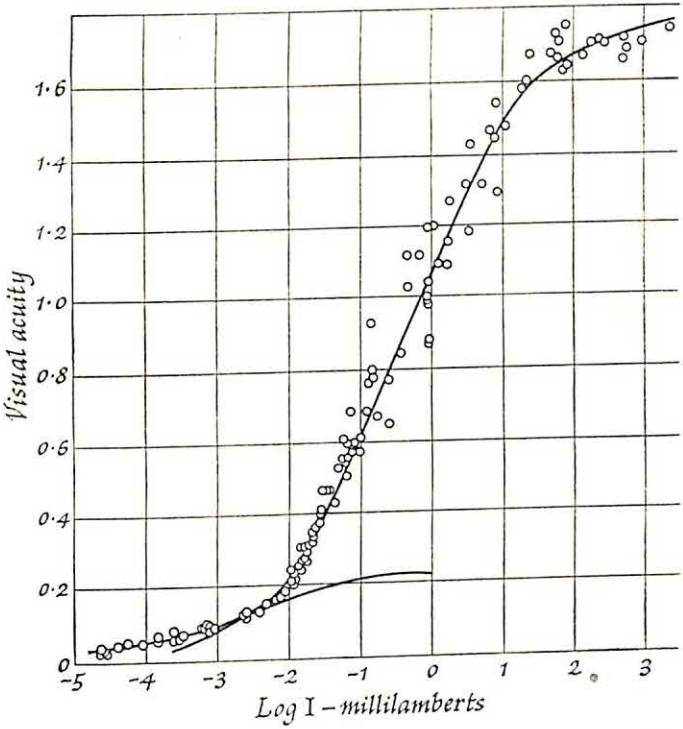

FIG. 1-10. Curves showing relationship between visual acuity and level of illumination (I). Circles show König's data. The two solid lines show, (lower) rods and (upper) cones, the success with which a normal probability integral can be fitted to the data.

of a normal curve, representing the thresholds of the retinal receptors. With increasing intensity more receptors would become functional and so the effective retinal 'grain' would become finer, giving higher acuity. This theory has many objections; in particular we know from electrophysiological and other evidence that the variance of receptor thresholds is not nearly great enough; if it were, about half the receptors would never normally be effective as acuity goes on increasing at very high intensities. This would mean that half the receptors are virtually useless.

Although the curve looks simple, there is probably no one simple explanation. The following factors are undoubtedly important:

(1) the number of quanta reaching the retina (see Pirenne and Denton), (2) the use of the fovea rather than the periphery. Not only is the receptor density greater in the fovea, but also the summation areas are smaller, (3) the development of lateral inhibition, giving smaller summation areas, which takes place with light adaptation.

2. *Acuity for one and both eyes.* Acuity may be compared for one and both eyes. It is better for both eyes, and the same is also true for intensity discrimination.

$\Delta I/I$ is less with two than with one eye, as may readily be shown (Pirenne).

3. *Acuity as a function of the number of possibilities.* This experiment involves a most important theoretical point. It is possible to think of sense organs in two ways: as receivers of energy, or as transducers of information. In either case energy is involved, but if we think of the eye (or ear) as conveying *information* then concepts beyond pure energy considerations become important. One might think of energy from the gap in the ring stimulating the retina and causing (after a lot of neural activity in the brain, etc.) a response. Alternatively, one might think of the retinal activity serving as information to increase the probability that the gap is at one rather than another of the possible positions in which it could be. On the former view the number of positions in which the gap might be found is irrelevant, but on the informational analysis it is very important. The question is: does the number of positions in which the gap *might* be found affect acuity? We leave it as an exercise to design an experiment to answer this question.

TEMPORAL DISCRIMINATION – FLICKER FUSION

Temporal discrimination would seem to be limited by intensity discrimination – in much the same way as for spacial acuity. In spatial discrimination the gradual falling off in intensity of the borders of images produces overlap of the images from adjacent objects: the difference in intensity between the 'trough' and the centre of the images must be distinguished. In flicker the 'trough' lies in time rather than space. The time constants of the eye are important in determining the 'temporal troughs' for given stimulus conditions.

The eye integrates energy over time. As we have seen, the Bunsen-Roscoe law states that, for time intervals up to about ·02 second, the eye is a perfect integrator. For detection a flash can be indefinitely short provided it is sufficiently intense: it is the product of time and intensity that matters. The same law holds (very nearly) for photographic emulsions, and in both cases it has a photochemical basis, though for emulsions there is an effective loss of speed for very short flashes, but this is because two processes are involved.

For the eye there is also *summation of information* in time. A very important question is how far the perceptual phenomena found for pattern recognition in a tachistoscope follow the Time × Energy relation which holds for the energy required to detect a flash. If information is, so to say, serially processed in perception, then we might expect a quite different relation (logarithmic?) to hold for tachistoscopic experiments involving pattern recognition.

Flicker itself is of comparatively little interest in normal life, but if we consider an image travelling across the retina, due to the movement of an object in the visual field, then it is clear that the rate at which each part of the retina can respond to differences in intensity may be important for the detection of the movement. The Time × Intensity relation is found to hold for the threshold for seen movement (Leibowitz 1955).

An eye which adopted a longer integrating time would, other things being equal, be more sensitive, but its temporal discrimination would be worse. There is evidence to suggest that in dark adaption the eye sacrifices temporal discrimination for sensitivity, the integration line becoming longer (Gregory 1955a).

Experiment to estimate critical flicker fusion frequency (c.f.f.). For measuring c.f.f. it is important to have an arrangement which maintains the intensity of the source constant for all rates of flash. The simplest method is a sector wheel (episcotister) revolving at controlled rates and arranged to interrupt the light from a fixed source.

A slide projector giving a spot of light on a screen whose area and brightness may be varied is most convenient. (The projector lamp will normally be run from mains and so will itself have a flicker component but this is small and may be ignored for most purposes.)

It will be found that c.f.f. increases with increase in (*a*) brightness, (*b*) area of flickering light, (*c*) area of background.

Discrimination – conclusions. A number of types of discrimination have been considered. Speaking generally, sensory discrimination seems to be limited by the ability of the nervous systems involved to detect small intensity differences. This is even true of acuity and colour vision. Thus intensity discrimination becomes of cardinal importance to the study of sensory processes.

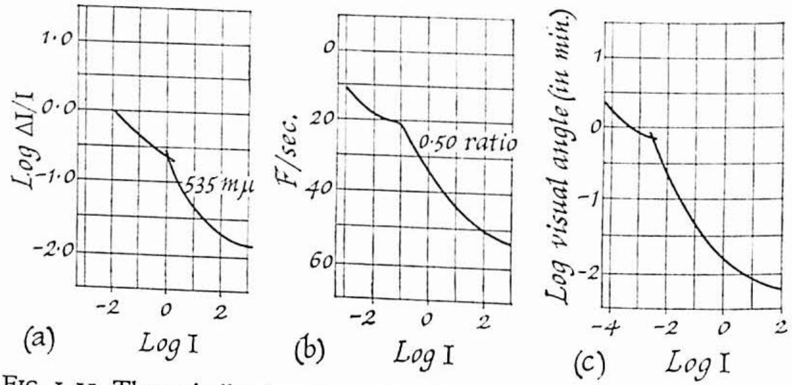

FIG. 1-11. Three similar functions where intensity is a variable: (*a*) brightness discrimination, (*b*) CFF, (*c*) acuity. (After Osgood.)

Communication and electronic engineers who design equipment to have maximum sensitivity – or discrimination of what is wanted against what is unwanted – have developed the mathematical language of signal/noise ratio. Here 'signal' is the wanted activity in the system and 'noise' is random unwanted activity tending to mask or destroy the signal. Now all nerve fibres display occasional bursts of activity in the absence of any external stimulus or 'signal'. There is no reason to believe that the nervous system is immune from the random disturbing influence of 'noise' which sets the limit to physical systems detecting small differences. Various attempts to measure the noise levels in the nervous system have been made (Barlow 1956, Gregory and Cane 1955, Gregory 1956). By thinking of sensory thresholds as limited by the signal noise ratio of the system we have an unusually powerful hypothesis which helps to relate sensory data with physiological mechanisms.

ADAPTATION TO INTENSITY OF LIGHT

The eye can function over a range of intensities of the order of 10,000 : 1. We are, however, temporarily dazzled and almost blind if we emerge suddenly from near darkness into bright light. This would seem to be a weakness of the system. Adaptation to dim light takes place comparatively slowly: sensitivity may increase for up to one hour in darkness. It is difficult to imagine that this long time course is of any biological advantage.

Since the work of Hecht (1934) it has been rather generally supposed that the retinal basis of adaptation is the density of the photochemical rhodopsin. It has recently been shown by Rushton and coworkers, with unusually beautiful experiments, in which the concentration is measured directly in the living eye, that the change in rhodopsin concentration with normal levels of incident light is very small. This implies either that rhodopsin concentration is unimportant for adaptation, or that the small changes which do occur have a surprisingly large effect on the quantum capture and thus on sensitivity of the eye (Rushton 1952; Rushton and Campbell 1954; Rushton 1956). Rushton's experiments give the only direct information on this matter, but that other factors influence sensitivity can be readily demonstrated. Not all of these are retinal in origin.

1. The diameter of the pupil increases in dim light, giving a 10 : 1 variation in light gathering power.

2. The retinal *area* over which summation takes place in the retina increases in the dark, this being due to decreased lateral inhibition (Barlow, Fitzhugh and Kuffler 1956).

3. The *duration* over which integration of energy takes place increases with dark adaptation (Gregory 1955*a*).

It would seem that many mechanisms are involved. These are very imperfectly understood, and until they are understood we are confronted with challenging fundamental problems.

Demonstration of dark adaptation. A simple demonstration, due to K. J. W. Craik, is to have a series of small windows each illuminated

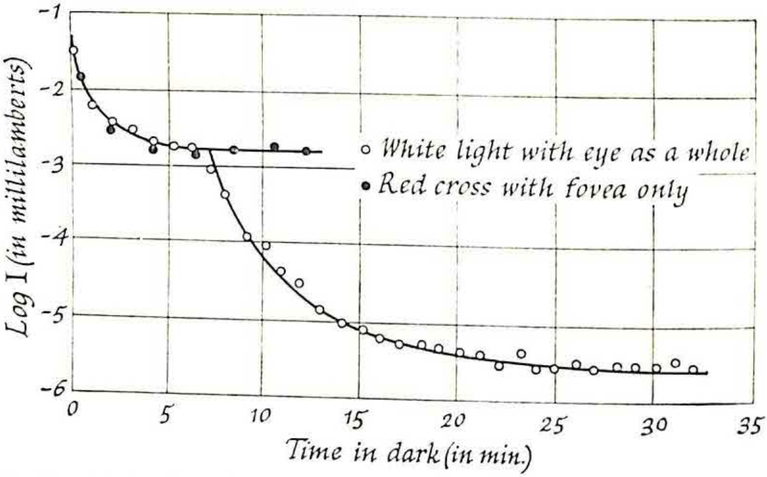

FIG. 1-12. Dark adaptation of the human eye, showing the increase in sensitivity with time in darkness for cones (solid dots: fovea only) and rods (circles: white light, eye as a whole).

with a different, low intensity. It will be found that they appear one by one in order of brightness if the display is viewed in a dark room.

At the present time adaptation is a mystery; until it is solved many other visual processes remain mysterious.

THE PERCEPTION OF MOVEMENT

If we consider the perception of the movement of a small self-luminous object viewed in an otherwise dark room, it is clear that there must be at least two distinct neurological systems, or 'channels', for the perception of movement. The movement of the light may be seen if (*a*) the eye is held still, the image travelling across the retina as the object moves across the field, or (*b*) if the eyes follow the object, in which case the image will remain stationary upon the retina, except for small tracking errors. We may call the first system – depending upon shift of the image across the retina – the *image/retina system*, and the second – depending upon tracking of the eye – the *eye/head system*.

A similar distinction may be made for tactile movement perception – if the skin of the hand is stroked by an object movement is experienced: it is also experienced if the hand is allowed to move with the object. Clearly, quite different neural systems are involved in these cases. This seems to be a matter of some importance.

Stability of the visual world during eye movements. The problem may be stated quite simply. If the eyes are moved (i.e. rotated) the images of objects in the field will be displaced across the retinae. Movement of images normally gives information of movement: why then, does not the visual world apparently move during eye movements?

It was originally thought that the stability of the visual world during voluntary eye movements was produced by proprioceptive feedback from the extrinsic muscles of the eye. These proprioceptive feedback signals would be used by some central mechanism to cancel the movement signals from shifts of the image across the retina. There are, however, serious objections to this 'inflow theory': (1) Neural feedback loops are almost certainly too slow, so that the cancelling signals would not arrive in time to prevent perceptual instability. (2) Passive eye movements (such as by pressure with the finger) would be expected to produce signals from the stretch receptors. These should have some discernible effect, but none has been detected in perception, though Cooper and Daniel and Whitteridge (1953) have recorded their activity in the goat. (3) Helmholtz pointed out that patients suffering from paralysis of some of the extrinsic eye muscles experienced movement of the visual field when they tried to move their eyes, though in fact the eyes remained stationary. The experienced movement was always in the direction the eyes should have moved. It was this observation – which Kornmüller (1930) has repeated by anaesthetizing the muscles with cocaine – which led Helmholtz to suggest the *outflow theory*, which is that the eye movement command signals are utilised for stabilization, presumably by an internal monitoring loop. (4) It is easy to show that passive eye movements do not shift after-images. This provides evidence against proprioception playing a part in stabilization, for some stretch receptor activity should be produced, and this should produce movement of the after-image.*

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* The best way of obtaining the after-image for this type of observation is to fixate an electronic flash tube, giving say 110 joule flash in about 1/1000 second, as used for photography. This is a very useful tool for visual experiments.

After-images usually move with the eye, even when viewed in complete darkness, during voluntary eye movement. Stability, therefore, does not hold for after-images: why should this be? The most probable answer is that under normal conditions perceptual stability is achieved by a cancelling of opposed signals from (*a*) an internal loop monitoring the command signals to the eye, and (*b*) from the shift of images across the retina. There should be equal and opposite signals from these during eye movements: if these were arranged to cancel stabilization could thus be achieved quite simply (Gregory 1958).

APPARENT MOVEMENT

1. The 'waterfall effect'. Probably all the sense modalities can be fooled. A flickering white light may appear strongly coloured – because of the different time-constants of the physiological colour systems – and coloured lights will be observed if the eye-ball is pressed fairly hard with the finger: this stimulates the so-called pressure phosphenes. Any sense organ may be stimulated faradically, that is, with a weak electric current. The sense organs also tend to be fatigued, or adapted, by strong stimulation. The eye does not see colours normally for several seconds or minutes after stimulation with a bright coloured light. The after-effects of a strong stimulus are perhaps most striking in the case of movement, though it is surprising how few people notice these effects until they are pointed out.

Demonstration of the 'waterfall effect'. An endless belt of transverse black and white stripes may be driven continuously (a polygraph recorder giving various speeds is suitable for this) and fixated for say 10 to 30 seconds. The belt is then stopped, and it will be found that it appears to *drift backwards* in a marked but rather odd manner. It moves, and yet it does not change its position. Quantitative measurements can be obtained most easily by timing the duration of the effect.

This effect is most marked if the eyes are kept fixed upon the belt, using a fixation spot which may be projected upon it. If the eyes are allowed to follow the belt the *real* movement is still seen (by the 'retina/head' system) but the *after effect* is greatly reduced if the eye follows the belt, and in many subjects is entirely absent. This suggests that it is not the perception of movement *per se*, but the stimulation of the image/retina system which produces the effect. It does not, however, follow that the effect is due to a retinal rather than a central change, though on the whole this does seem the more likely. (Cf. Wohlgemuth 1911).

2. The phi-phenomenon. This is historically important in Gestalt Theory. The effect is familiar from the apparent movement experienced in illuminated signs. The conditions required for this apparent movement concern primarily the distance of the flashing lights and the interval between the first light going off and the coming on of the second light.

Wertheimer (1912) argued that this form of apparent movement is due to spread of activity in the cortex, a view suggested by the Gestalt theory of *isomorphism*. This theory is now generally abandoned, and the origin of the phi-phenomenon would be looked for first at the retinal level. It seems likely that what we have called the image-retina movement system is activated by flashes of light occurring within appropriate intervals and distances (see Chapter 2).

3. The autokinetic effect. This also is historically important in Gestalt Theory. It is the apparent movement of a small dim spot of light observed in an otherwise dark room, or any small spot against a homogeneous field. Koffka (1935) says of it:

> These 'autokinetic' movements, then, prove that no fixed retinal values belong to retinal points; they produce localization within a framework, but do so no longer when the framework is lost. . . The autokinetic movements are the most impressive demonstration of the existence and functional effectiveness of the general spatial framework, but the operation of this framework pervades our whole experience.

Koffka stressed the importance of perceptual 'reference frames' in stabilizing the field, in this special case where the framework could only be conceptual because it had been shown that the autokinetic movements were not, as had previously been thought, due to eye movements. Guildford and Dallenbach (1928) showed, photographically, that suitable eye movements do not occur under these conditions. We may check this in a simple manner.

Experiment to discover whether the eye moves during the autokinetic *effect:* the fovea is blind to dim blue light (Willmer 1946). If we view a small dim blue patch of light it disappears when it is fixated. This effect may therefore be used to detect eye movements.

A set of Ilford Narrow Cut Spectral Filters is obtained, and a hole punched in the centre of the Blue filter. With the same punch, a disk of the same size is obtained from the Red filter, mounted in the hole in the Blue filter and bound between glass slide covers. The outer Blue filter is then masked with a hole of about 1 inch diameter. This arrangement is mounted in the side of a light-tight box fitted with a torch bulb. The intensity may be varied with a small variable resistance (Gregory 1959).

If this arrangement is viewed in the dark, nothing but a red disk of light will be seen with central regard, but if the eye is moved slightly, a bluish-white halo will appear round the red spot, as the blue field becomes visible. With dark adaptation this effect becomes quite marked. The subject is required to note whether this halo appears during autokinetic movements of the red spot.

The answer is, it does not. Thus the effect cannot be due to slow movements of the eye.

Experiment to produce induced 'autokinetic' movements. If the eyes are held hard over to one side, or up or down, for from 10 to 30 seconds before viewing the red spot, it will be found that, generally, there is a very fast drift of the spot in the opposite direction from the previous eye movement. Evidently the muscular imbalance sets up a related apparent movement. The same thing happens with tension applied to the neck muscles, as may be shown by pushing the head backwards or forwards, or rotating it on a vertical axis right or left. It may be suggested that in these cases we have upset the neural movement channels which normally, in the absence of external movement, cancel out during eye movements. We seem to have upset the stability system which is so important for distinguishing between the movements of the body or eye from movement of external objects. The autokinetic effect may be due to small random disturbances of these very complicated and delicately balanced systems (Gregory and Zangwill 1962).

COLOUR VISION

The eye is sensitive over a range of wavelengths from about 400 mμ to about 700 mμ. The photophic sensitivity curve shows the sensitivity of the eye to wavelengths along the spectrum, but in addition different colours are seen as wavelength is changed, though this does not appear from the luminosity curve, which ignores colour, being concerned only with sensitivity and wavelength.

The seven spectral colour names – Red, Orange, Yellow, Green, Blue, Indigo and Violet – were given by Newton.* He originally adopted five, adding Orange and Indigo because he liked the number seven. Few people would spontaneously use, or feel the need for, these additional names, particularly Indigo.

It is impossible in experiments on colour vision to require the subject to name each colour he sees, because it is quite impossible to name the several hundred discernible hues consistently. Colour experiments – apart from certain demonstrations – generally involve matching a field composed of a monochromatic or mixture of lights with some other mixture, or under some other condition, and requiring the subject to state when the fields appear identical. The data are then in terms of the various mixture components, or other conditions, required to give a match.

Thomas Young (1807) made the most important discovery after Newton's work on light. Young found that any spectral hue may be produced (and matched by) a suitable mixture of three and only three coloured lights. Further, the choice of these three colours (wavelengths) was found to be not critical. Young wrote:

> As it is almost impossible to conceive of each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in unison with every possible undulation, it becomes necessary to suppose the number limited, for instance, to the three principal colours, red, yellow and blue . . . and each sensitive filament of the nerve may consist of three portions, one for each principle colour (1802).

Young later changed his 'principal colours' (1807) to red, green and violet.

* These may be memorized with the mnemonic: 'Richard Of York Gained Battles In Vain'.

Helmholtz's modification of Young's hypothesis gives us three types of receptor (cones), each type having its own spectral sensitivity curve. The colour seen depends on the relative activity of the three systems. Since the sensitivity curves must overlap, it is not possible to stimulate one curve at a time (except perhaps for far blue or far red light) and so the problem of discovering the form and spectral position of these hypothetical curves turns out to be very difficult. Four principal techniques have been adopted: (1) to record the activity from single receptors with micro-electrodes placed on the giant ganglion cells at the front of the excised retina, (2) selectively fatiguing the eye with bright coloured lights applied to one eye and making binocular matches under special mixture conditions, (3) to study colour vision in cases of colour anomaly, (4) measurements of hue discrimination.

The reader is referred to Granit (1947) or to Morgan and Stellar (1950) for a summary of Granit's work on the electrophysiology of the retina. The adaptation work was mainly undertaken by, and is described by, Wright (1946), where the colour anomaly evidence is also given. This is reviewed by Brindley (1957).

In addition to *colour* the receptors also transmit information of *brightness* and *saturation*. A colour is desaturated by the addition of white light: if brightness were given by special cones having roughly the overall photopic sensitivity curve of the eye, then saturation might be given by the relative activity of this brightness receptor and the colour cone systems. If white light were then added to a monochromatic light this would produce desaturation of the colour by adding brightness with only slight increase of activity in the colour systems being stimulated by the monochromatic light. The evidence for such a brightness receptor is, however, not conclusive.

Demonstration of colour mixture with three coloured lights. If three slide projectors are available, with some form of intensity control, they may be fitted with aperture stops and their lights combined on a screen, giving three overlapping disks of light. A different colour filter is then placed in each beam. The Red, Green and Blue Ilford Spectral Filters are ideal.

It will be found that with suitable adjustments of relative intensity the portion where all three beams overlap can be made white. Change in the intensity of the colours will change the mixture to any desired spectral hue. Brown and black (which are not present in the spectrum) cannot be produced.

If a Yellow filter is placed in one lantern, and the Red and Green in the remaining lanterns, it will be found that the monochromatic yellow can be matched with these two (red and green) lanterns. This turns out to be important.

This finding that spectral hues can be produced with only three (but a minimum of three for the normal eye) lights does suggest that all colours are seen by the combined activity of three basic colour systems, but it does not *follow* that there are only three colour systems. How can we find out whether there are more than three systems?

Three is the *minimum* possible number. The maximum number would be one colour system for each discriminable hue, that is, any given monochromatic light would stimulate only one set of receptors which would be unaffected by all other wavelengths. This would require several hundred sets of receptors, or colour systems: one for each hue. It is quite clear that the eye is not like this. If it were, visual acuity in monochromatic light should be extremely poor (impaired by a factor of at least 100) if no receptors but those sensitive to a given narrow band of wavelengths were functional. It would have to be a narrow band because of the several hundred hues we can distinguish without – on this hypothesis – allowing combined activity from different systems to play a part in colour vision. Visual acuity in monochromatic light is very nearly equal to acuity in white light of comparable brightness; except for the case of blue light when acuity is poor, but this is due to the blue-blindness of the fovea (Willmer 1946).

It is clear that there must be at least three systems, but there cannot be very many more than three or acuity in monochromatic light would be very poor due to the effective coarsening of the retinal 'grain'. The main problem in colour vision is to determine how many colour systems there are, and what their spectral sensitivity curves may be.

Hering postulated a special yellow receptor, largely because yellow seems to be a unique experience. This is no argument: the simplicity or uniqueness of an experience is no guide to the degree of complexity of the physiological processes subserving it. In any case, we know that yellow *can* be produced by red and green mixture. There would thus have to be *two ways* of seeing yellow: by the special yellow receptor and by the systems primarily serving red and green. We can show in a simple class experiment that there are not two ways of seeing yellow and this is a matter of considerable importance.

Experiments to discover whether there is a special yellow receptor. We require some form of anomaloscope. This does not have to be of high class, or accurately calibrated. A simple filter instrument would serve. An anomaloscope is an instrument designed to measure the relative intensities of Red and Green light required to match Yellow light. It provides two adjacent comparison fields, one of these fields being produced by monochromatic yellow light, and the other by a mixture of red + green light in adjustable proportions. The situation is thus exactly like that of the three lanterns described above, but with a yellow filter in one lantern providing one field and a red and a green filter in the others to give the second comparison mixture field. The instrument is normally used as a test for colour anomaly, the common kinds of colour anomalous (or 'colour blind') eyes requiring unusual proportions of red and green to match the yellow, as discovered by Lord Rayleigh in 1881.

All that we require beyond the anomaloscope is a bright source of light, and a red and a green filter.

The subject first carefully adjusts the relative intensities of the red and green lights so that the mixture field matches the monochromatic yellow field. The whole display will then look the same yellow. He then looks at a bright source of light through, say, a red filter. This fatigues, or adapts, the eye to red. If he now looks back at the anomaloscope fields, they should appear green. (If this is not the case, the adaptation was insufficient, and greater intensity or time is required.) Now the question is: with this selective colour adaptation, do the two fields still look alike? Is the match retained?

If the monochromatic yellow stimulates a special yellow receptor the pre-adaptation to red should not affect the perception of yellow by *monochromatic light* to the same extent, or in the same way, as the perception of yellow by *red and green mixture*. If, on the other hand, yellow is always seen effectively by mixture, from the red and green systems, then the monochromatic field should appear exactly the same colour after adaptation as the mixture field now appears.

The answer is that both fields *are* equally affected, suggesting that both stimulate the same retinal systems. For extremely bright adapting light there is some shift, but this is too small to affect the argument, and the shift is always in the same direction, towards red (Wright 1936; Brindley 1953; Gregory 1955b).

It is possible to see a fairly good yellow by stimulating one eye with red and the other with green light. If the intensities are suitably balanced, at least some observers will occasionally see yellow, though there is a great deal of retinal rivalry, producing alternating red and green. When yellow is seen this must be due to central fusion of activity from red and green systems.

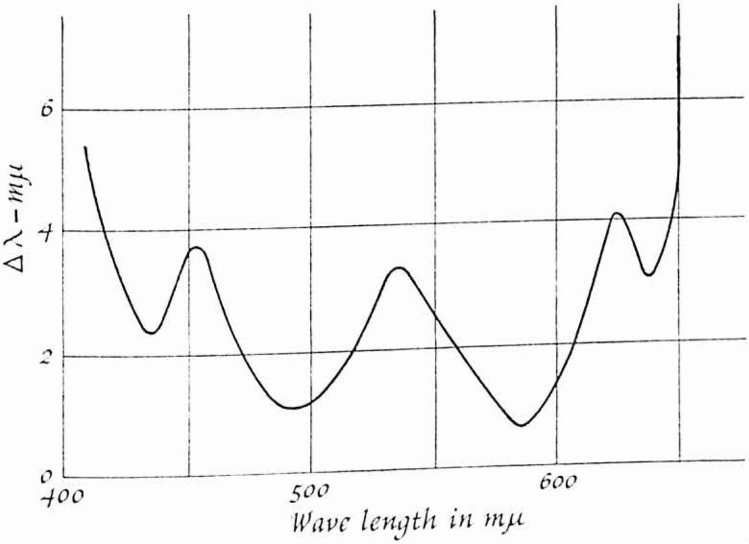

FIG. 1-13. Hue discrimination. (Steindler 1906.) This shows by how much wavelength must be changed ($\Delta\lambda$) for a detectable difference in hue for each position along the visible spectrum (λ). This curve gives perhaps the most fundamental information obtainable by psycho-physical means for discovering the response systems giving colour vision.

Hue discrimination. Hue discrimination is of practical importance in the dyeing and paint industries, as well as for the theory of colour vision. It is measured by changing the spectral wavelength, and recording the change in wavelength required for a difference in hue to be seen, brightness being kept constant. This is technically too difficult for a class experiment, but the implications of the *hue discrimination curve* should be considered.

Fig. 1–12 shows a hue discrimination curve determined by Steindler (1906) for normal vision. It will be noted that $\Delta\lambda$ (the difference in wavelength required to detect a difference in hue) is plotted against λ (wavelength). In this curve there are four minima: 440, 485, 575 and 625 mμ. These are the spectral positions where hue discrimination is best.

Given that there are several cone systems, each having its own sensitivity curve, the hue discrimination curve should correspond to these primary response curves in the following way. Consider first the high values of $\Delta\lambda$ at the extreme red and blue ends; at either end only one colour system will be functional, and change of wavelength should produce only a change in brightness. This is the case for red over quite a wide range. As wavelength is reduced towards yellow, the next receptor system (green?) will become stimulated, and the hue will depend on the balance of outputs from these two systems. Now presumably $\Delta\lambda$ will depend upon the relative slopes of the fundamental response curves for each value of λ. Thus, there should be a relation between the hue discrimination curve and the fundamental response curves. This is probably the most direct information we possess of the form of the fundamental response curves, but unfortunately rather different hue discrimination curves have been obtained by different workers and, apparently, from different subjects having similar vision.

It is important to note that hue discrimination is limited by the minimum *intensity difference* which can be detected between the fundamental colour systems. Intensity discrimination again turns out to be of basic importance.

Colour anomaly. More usually, though misleadingly, referred to as 'colour blindness', colour anomaly is essentially an inability to distinguish certain colours which are readily distinguished by the majority of observers under the same conditions. The most common form of confusion is between red and green. Its discovery is usually attributed to the chemist John Dalton, in 1794.

Lord Rayleigh (1881) discovered that people who are apt to confuse red and green require unusual amounts of red or green in a red plus green mixture field to match a monochromatic yellow light. It was this finding which led Nagel, in 1907, to develop the anomaloscope, used as a test of anomaly. Those observers requiring unusual matches are known as *anomalous Trichromats*. This means that they, like normal observers, require three lights to match the spectral hues, but that the proportions of red and green required to match yellow are unusual. Some observers require only two lights to match all the spectral hues they can discern: these are called *Dichromats*. Occasionally *Monochromats* are found who have no ability to distinguish colours apart from their intensity differences. Their world is like a black and white film.

The following classification may prove helpful:

I. TRICHROMATS	II. DICHROMATS	III. MONOCHROMATS
1. Normal colour vision	1. Protanopia	
2. Protanomaly	2. Deuteranopia	
3. Deuteranomaly	3. Tritanopia	

Now what does this mean? Section I observers (Trichromats) all require three mixture lights to match the spectral hues, but protanomalous observers require more red, and deuteranomalous observers more green, to match yellow. In Section II (Dichromats) only two mixture lights are required. Protanopes do not require red, deuteranopes green, and tritanopes (rare) blue. Monochromats are of only one kind – no colour vision.

HEARING

The physics of sound. A vibrating diaphragm or string, or a column of air as in an organ pipe or the larynx, alternately compresses and rarefies the air round it.

When the increase in pressure is replaced by a local decrease, a pressure wave is replaced by a 'trough'. A diaphragm, such as the drum of the ear, may be displaced back and forth with the alternating pressure waves. When this is suitably attached to the mechanism of the middle ear, and to the neural mechanism of the cochlea and the brain, rapidly alternating pressure changes may be heard as sound.

The ear can hear tones over a frequency range from about 30 c/s–20,000 c/s. Frequencies below 30 c/s are heard as vibration rather than tone. The upper limit of 20,000 c/s is reached only by children and young adults. There is a continual falling off in the upper frequency limit of hearing with age (Presbiacusia). The useful upper limit might be put at 16,000 c/s.

The energy in a simple (sinusoidal) sound wave. The energy of sine waves can be measured in terms of *peak amplitude*, or in terms of *root-mean-square amplitude*. The latter is generally used, because it is a more stable value for complex waves, and we require to measure energies in complex as well as sine waves. In the case of sine waves, there is a very simple relation between these two measures. Where P is the pressure:

$$P_{rms} = \frac{P_{\max}}{\sqrt{2}} = 0{\cdot}707P_{\max}$$

No simple relation holds for complex waves. We shall use *rms* values, and this is standard practice.

The intensity range of the human ear is extremely large; it covers a range of about $10^{12} : 1$. (This is, of course, 1,000,000,000,000 : 1.) The threshold of detection is only about 10^{-16} watt.

Relative intensities are given in *decibels* (db). This is a very useful measure, and is defined as:

$$N \text{ (decibels)} = 10 \log_{10} \frac{P^2}{P^2_{\ r}} = N = 20 \log_{10} \frac{P}{P_r}$$

or

$$N = \frac{\log E_2}{\log E_1}$$

What does this mean? N refers to the number of decibels (where 1 decibel = 0·1 bel); P is the *pressure of the tone being* measured, and P_r the *pressure of some standard reference tone.* E is energy, or P^2. It should be particularly noted that the decibel unit is not an absolute unit, such as the metre, the second, or the gramme. The decibel is a *relative* measure. Any decibel value (e.g. 30 db) always means '(30 db) above, or below, some other intensity'. In hearing, we frequently find that no reference value is given explicitly. Thus we may read: 'a 30 db tone of 1000 c/s was presented to the left ear'. This can be said, although the decibel is only a relative measure, because there is a generally agreed conventional origin (or P_r) to the scale. This is the absolute threshold for the average human ear for young adults. The value of this threshold has been determined, and is conventionally accepted as 0·0002 dyne/sq. cm for a 1000 c/s tone.

Complex waves – harmonics, beats. To get a feel for the physics of sound it is almost essential to have the use of two or three audio-frequency oscillators and a suitable oscilloscope. Arrangements should be provided for mixing the signals from the oscillators. In addition, a tape recorder with microphone (preferably a twin-channel stereophonic recorder), and various musical instruments, such as tuning forks, organ pipes and a stringed instrument, are extremely useful.

The use of an oscilloscope. A cathode ray oscilloscope is an instrument similar to a television receiver. A beam of electrons appears as a spot on a fluorescent screen at the end of the tube. This spot is repeatedly deflected at constant rate across the tube, from left to right, and is very rapidly returned. It is swept across by the *time-base*, the return being known as the *fly-back*. In the absence of incoming signals, a horizontal line is seen traced out on the tube face by the scanning spot. It is seen as a line because of persistence of vision, and because of the time constant of the 'phosphor', i.e. the fluorescent substance. Incoming signals are in the form of voltages, which are generally amplified by internal amplifiers whose gain must be suitably controlled. A change in the input signal voltage causes the spot to move up or down, and as it is carried along at constant rate (hence the expression 'time-base') a wavy line is seen which represents the changes of the signal voltage in time. The amplitude of the wave is linearly related to the voltage. If a microphone is attached, pressure changes in the air may be seen as waves – a dynamic graph – on the screen. A tuning fork, or the output from a single oscillator, will give a sine wave. What is actually seen is a number of sine waves, or perhaps part of one, depending on the setting of the time-base frequency control. Suppose we wish to look at a 1000 c/s wave: if the repetition rate of the time-base is 1000/second, we shall see one sine wave, minus the bit missing during the fly-back time. With a repetition rate of 500/second, we shall see two sine waves, and so on. If the time-base repetition rate is not equal to, or a submultiple of, the signal frequency, then the display will run across the screen – an endless chain of sine waves chasing each other, but never catching and never caught.

If the outputs from two oscillators are mixed, and the result viewed on the 'scope', different patterns are produced as the intervals between the tones are changed. At any moment when they are *in phase* they will *add together* producing a greater deflection of the spot; when *out of phase* the spot will tend to return to the centre line of the screen. Two waves of the same form and amplitude but opposite phase would produce a straight line, and silence. Zero amplitude (and silence) is produced at regular intervals by two waves of different frequency but the same amplitude, at those moments when they have equal and opposite values. If we set one oscillator to 1000 c/s and the other to 1010 c/s, we shall hear this happening every tenth of a second, and the result is a *beat*, which in this case will be heard as a throb rather than a tone.

A vibrating string, organ pipe or other instrument, gives a *fundamental frequency* and *overtones*, or *harmonics*, which are always multiples of the fundamental. The different timbre of musical instruments is due to the various relative amplitudes (or energies) of the harmonics. Instruments such as oboes and stringed instruments are rich in harmonics, while tuning forks and flutes are not; they give almost pure sine waves. Pipe organs can synthesize the various timbres of instruments by providing the appropriate harmonics from their range of pipes. These are controlled by the stops. Any wave, however complex, can be built up out of a set of sine waves of various amplitudes and frequencies. Similarly, any complex wave can be analysed into a set of simple sine waves.

The structure and function of the ear. Pressure changes enter the ear through the *auditory canal* (*meatus auditorius*) and reach the *ear drum* (*tympanum*) (Fig. 1-14). This is an oval membrane placed obliquely in the meatus. It vibrates with the pressure changes reaching it from the sound source. Attached to the inner side of the tympanum is the first of three tiny bones or *ossicles*. These are (*a*) the *hammer* (*malleus*), (*b*) the *anvil* (*incus*), (*c*) the *stirrup* (*stapes*). These form a set of levers which reduce the amplitude of vibration of the drum. The further end of the stirrup (the footplate) is attached to a second membrane, the *oval window*. This forms the boundary between the *middle ear* (containing the ossicles) and the *inner ear* in which lies the *cochlea*. The cochlea is a snail shaped cavity in the temporal bone. If we take a cross section of one of the turns of the cochlea (Fig. 1-15a) we see three chambers divided by two membranes: *Reissner's membrane* and the *basilar membrane*. The three divisions, or tubes, running along the inside of the cochlea are the *scala vestibuli*, the *scala tympani* and the *cochlea duct*. The cochlea duct lies between the vestibular and tympanic scalae. Within it, and lying on the basilar membrane, are the receptor *hair cells*. They lie in rows in the *Organ*

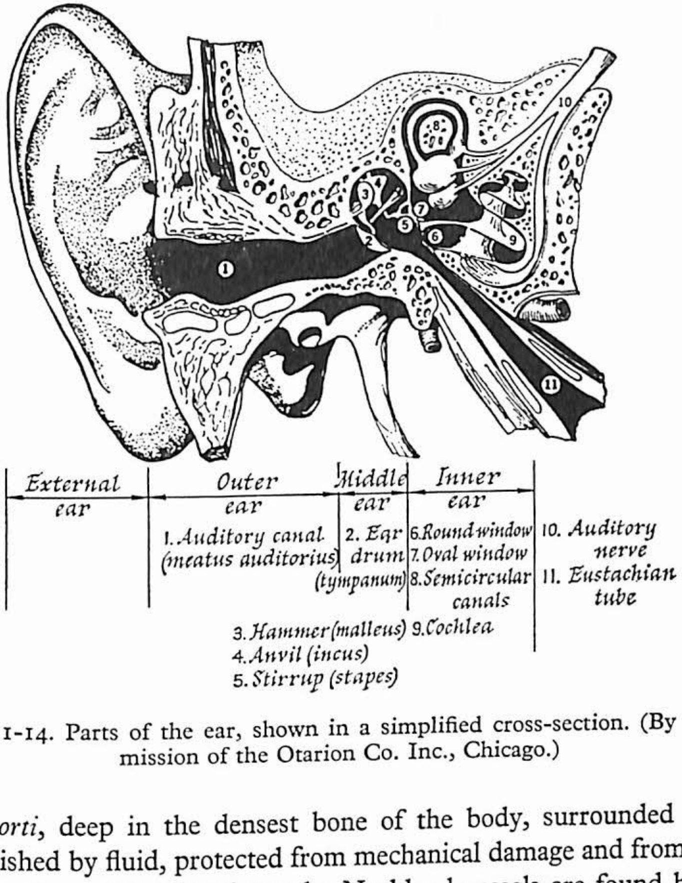

FIG. 1-14. Parts of the ear, shown in a simplified cross-section. (By permission of the Otarion Co. Inc., Chicago.)

of Corti, deep in the densest bone of the body, surrounded and nourished by fluid, protected from mechanical damage and from the fluctuations of the blood supply. No blood vessels are found here. The hair cells lie bathed in *endolymph* fluid, from which they are nourished. The freedom this gives them from pulsatory disturbance is no doubt an important factor in the extremely high sensitivity of the ear.

The scalae vestibuli and tympani are joined by an opening, the *helicotrema*, at the apical end of the cochlea. We shall see the significance of these tubes (which are filled with a fluid, the perilymph) with their common opening, and the significance of the reduction in amplitude of airborne vibrations from the drum to the oval window by the ossicle lever system.

The manner of vibration of the drum is important theoretically.

Its amplitude of movement is small – only about ·54 mm peak amplitude for the extremely high pressure of 1000 dynes/sq. cm at 100 c/s. This amplitude will fall off linearly with increase in frequency, until at 10,000 c/s it will be only ·00054 mm.

The ossicles as a mechanical matching transformer. The function and movements of the ossicles are discussed in detail by Wever and Lawrence (1954). The ratio of input to output amplitude for the cat ossicles turns out to be 2·5 : 1, but owing to the different areas of the drum and the oval window, the effective ratio is 60·7 : 1. For the human ear the ratio of the ossicle input–output movement is 1·31 : 1 and the total effective impedance ratio is 18·3 : 1. Now why is this matter of importance? The reason is that the sound energy is converted from vibrations in air to vibrations in a comparatively dense fluid, the perilymph, and for high efficiency of energy transfer some kind of *transformer* is needed to match the two impedances. The problem is exactly the same as that of matching the loudspeaker of a radio set to the output valve, though in this case the transformer is electrical, while in the ear it is mechanical.

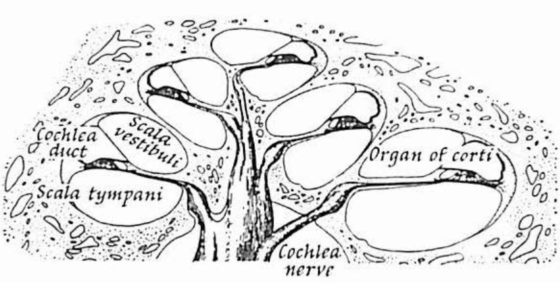

FIG. 1-15. (*a*) The cochlea shown in cross-section. (After Békésy 1957.)

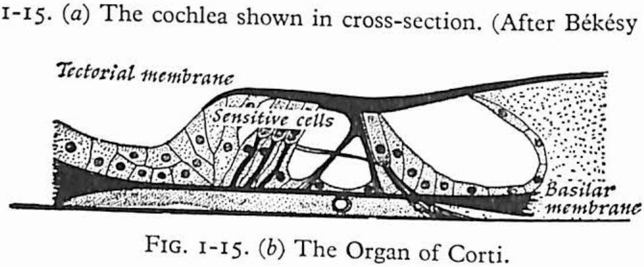

FIG. 1-15. (*b*) The Organ of Corti.

The impedance of the cochlea fluid changes with frequency, so that the optimum matching ratio will depend on the frequency. The ear is most sensitive at about 1500 c/s; at this frequency the acoustic efficiency is 38 per cent (a loss of 4·2 db). At 100 c/s the efficiency is only 3·5 per cent (a loss of 14·6 db). It appears that the effective mismatch of the middle ear system at low frequencies is largely responsible for the falling off in sensitivity below 1000 c/s.

The transmission of sound in the cochlea. Sound is no longer airborne after the oval window; it is a series of pressure waves in the perilymph. These travel up the scala tympani, through the helicotrema and down the scala vestibuli, which terminates in a membrane at the *round window*. (A frequency diagram of a schematic cochlea is shown in Fig. 1-17.) The pressure waves produce vibration of the basilar membrane which probably sets up a shearing force in the Organ of Corti, between the basilar membrane and the tectorial membrane (Békésy 1957). This shearing would serve to provide greater force (with a sacrifice of amplitude) to deflect the *hair cells*, the mechanical stimulation of which initiates impulses in the *auditory nerve*. From this point on the sound waves are represented in terms of neural activity; the Organ of Corti may thus be thought of as a *transducer* changing the sound waves into a form which can be handled by the nervous system.

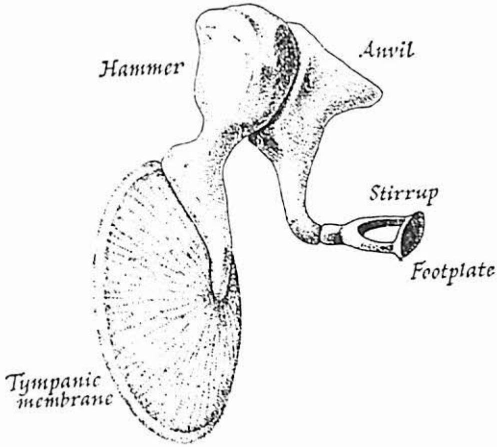

FIG. 1-16. The ossicles of the inner ear. The tympanic membrane is vibrated by airborne sound; these vibrations are passed through the lever system of the ossicles to the footplate of the stirrup, which is surrounded by a narrow membrane enclosing the perilymph fluid which carries the energy to the Organ of Corti.

The neural activity has been recorded and related to sounds of various kinds. It appears likely that for low frequencies the hair cells serve as counters: one impulse being generated with each complete cycle.

Unlike colour vision, we can isolate the separate tones in a chord, though the eye cannot provide any information of the component lights producing a colour mixture. It appears that the various frequencies are represented in different regions of the basilar membrane, as shown diagrammatically for the guinea-pig ear in Fig. 1-17. This spatial representation of frequencies has been established by a number of techniques including: (1) damaging regions of the cochlea with fine drill holes, and testing for specific deafness, (2) subjecting the ear to extremely loud pure tones over long periods and looking for related local damage, and (3) recording from different regions while the ear is stimulated with tones of various frequencies. These studies show that at least middle- and high-frequency tones are

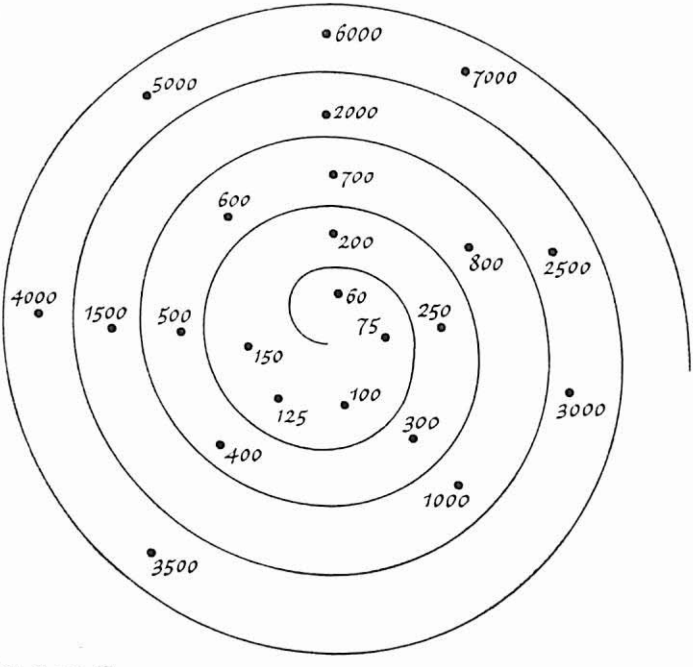

FIG. 1-17. Frequency map of the guinea-pig cochlea. (Culler *et al.*)

localized spatially, but that there is considerable spread of activity around any locus of primary stimulation. The physics of the cochlea as a system which localizes frequencies to specific regions is still open to doubt. Helmholtz suggested that it works by the principle of *resonance*; this can perhaps most easily be demonstrated with a piano. If the dampers are removed from the strings, by holding the loud pedal down, a piano will 'answer' any loud tone by sympathetic vibration of those strings which give the same tone when struck. Helmholtz suggested that the basilar membrane is tuned to different frequencies along its length, much as the piano is tuned to different frequencies from bass to treble. Békésy has, however, provided strong evidence against this view: he cut the membrane, while observing it under a microscope, and found that the cut ends did not fly apart. It thus appears that the membrane is not under tension, and so it could hardly be 'tuned' in the manner of a stretched string, or system of strings. He also found, by direct observation, that there could be a phase lag up the cochlea greater than one cycle; this could not occur with a resonant system.

Several attempts have been made to construct models of the cochlea. Békésy (1956) has considered the problem in terms of a travelling wave in a system of varying stiffness. He used a set of pendulums mounted on a rod which was periodically rotated, and investigated the properties of such a system under various conditions of coupling between the pendulums. This is a good example of the physical model approach to physiological problems; the actual arrangement used is quite simple, and should make an interesting demonstration.

It has been suggested that the cochlea is a 'wave guide', a system which has been studied in connection with the very high-frequency radio waves used for radar. It appears that wave guides have properties which might be used for producing separation of frequencies.

The finding that there is considerable spread of activity either side of the central locus of vibration of the basilar membrane raises the question: why does a loud pure tone not sound like a cluster of neighbouring tones? A possible answer is that the ear adopts gradient sharpening, as does the retina. This may indeed be a rather general property of sensory networks. If a sharpened matchstick is pressed into the skin of the hand or arm, until a marked depression is produced, the sensation is of a simple point. But since the surrounding skin has been pressed inwards, the surrounding touch receptors must be stimulated. It appears that activity from the peripheral receptors is rejected (inhibited). On this view of suppression of activity from less stimulated neighbouring receptors, we should expect that a tone might suppress, or mask, a neighbouring weaker tone.

MASKING EXPERIMENTS

1. Masking by tones. This experiment requires an audiometer and a separate audio oscillator. The signals from these are mixed into a common earphone.

The oscillator is set to a frequency which is not a multiple of any of the spot frequencies given by the audiometer, for we want to avoid the complication of harmonics; 800 c/s is convenient. Its intensity is set rather high, say 80 db. This may be found with sufficient accuracy by comparison with the audiometer. To do this, connect the outputs into separate 'phones, place one 'phone against each ear, and with the audiometer set to the nearest frequency to the desired *masking tone*, and giving an 80 db output, adjust the oscillator until the intensities balance. It may be advisable to reverse the earphones in order to check that the observer's ears are roughly equivalent.

An audiogram is then obtained (using 5 db steps for a class experiment) while the 80 db 800 c/s tone from the oscillator is mixed with the audiometer signal. If this is repeated with various intensities of masking tone, a family of curves such as those shown in Fig. 1-18 should be obtained.

It will be noted that (*a*) the only masking *below* the frequency of the masking tone is very close to that tone, and (*b*) there is a marked rise in the threshold for several octaves *above* it, with a depression at each octave.

If now the same procedure is adopted, but for *binaural masking*, where the audiogram is obtained from one ear while the masking tone is applied to the other, very little masking will be found. It is generally thought that any effect which may be discovered is due to sound crossing the head by bone conduction. It appears that masking takes place in the ear and not across the ears.

The reason for masking being almost entirely upwards in frequency may be due to some property of the cochlea, or it may be due to distortion in the ear (probably in the middle ear) producing harmonics from the fundamental masking tone.

2. *Masking by 'white noise'*. The same procedure may be adopted, but using in place of the masking tone a source of random or 'white noise'. (The curious term 'white noise' is taken over from optics, for white light consists of all frequencies in the spectrum. A monochromatic coloured light is analogous to a pure tone in this respect. White noise sounds like a hiss, and indeed the sound of escaping steam is essentially white noise.)

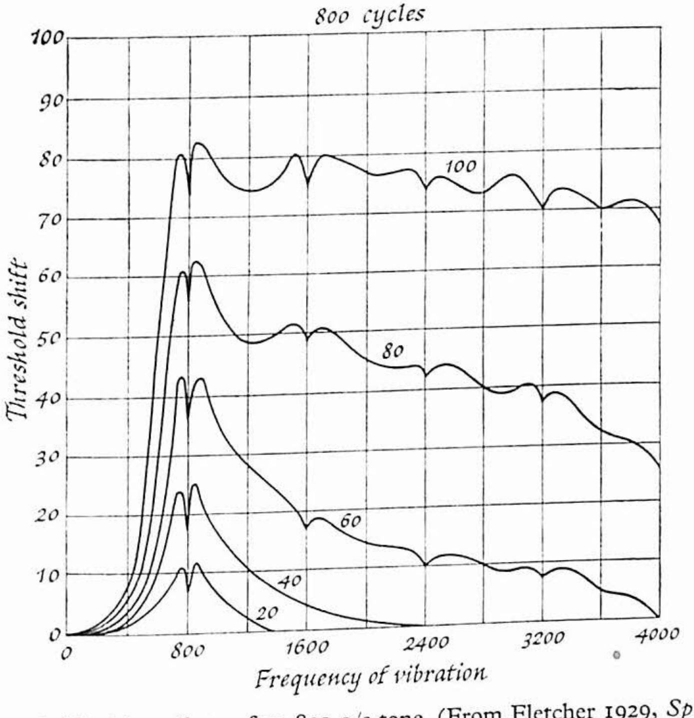

FIG. 1-18. Masking effects of an 800 c/s tone. (From Fletcher 1929, *Speech and Hearing*.)

The results for noise masking are similar to tone masking, in that masking increases with frequency, though continuously.

Localization of sound. People who are totally deaf in one ear are handicapped in their ability to localize the position in space of a source of sound. If a sound originates from one side of the head, it will provide more energy to the nearer ear, provided the frequency is fairly high. The reason for the difference in energy (and so of loudness) is that the head shadows the sound to the further ear. Long wavelengths of sound get round the head; there is hardly any shadowing for long, low-frequency waves.

Demonstration of the frequency-selective effect of the head shadowing sounds. If a noise source placed to one side of the head is heard, (*a*) with the nearer ear occluded, say by the palm of the hand, and then (*b*) with the further ear similarly occluded, it will be found that the noise sounds higher (i.e. with greater energy of high-frequency components) in the *nearer* ear. This demonstrates that the shadowing is frequency-selective, the higher frequencies being shadowed by the head and so lost to the further ear.

It may seem to follow that sound localization should be relatively poor for low-frequency sounds, but this is not the case. At low-frequencies the ears make use of the *phase difference* between the ears due to their different distance from the source. Any given change in pressure reaches the nearer ear first, and throughout a wave this time difference will remain constant, provided the direction of the source from the head is maintained. The simplest way of investigating binaural time discrimination is by applying pairs of clicks separated by various time intervals, one to each ear. Wallach, Newman and Rosenzweig (1949) found that time differences as small as 30–40 μsecond provided information of direction, and only one click is heard; with intervals greater than 2–3 m.seconds a double click was heard and localization lost.

The best way of experimenting with these important and interesting effects is with a twin channel tape recorder. Clicks at any desired time intervals may be produced on the two channels, by using two microphones so placed that the one to give the later click is the correct distance further from the sound source than the other microphone. If a single click is now produced, this will be recorded as one click on each channel. These will be spaced apart in distance along the tape, and in time when played back separately to the two ears.

It is found that phase difference is used for sound localization for frequencies below about 1500 c/s. Localization is rather poor for intermediate frequencies where, in other respects, hearing is most efficient.

Marler (1955) has provided evidence that in animals warning cries tend to lie in the range where localization is poor, thus the 'sentry' is not given away, but such cries as mating calls are in a region where localization is good. Thus the position of the suitor is given away, but the mate is not frustrated, at any rate for auditory reasons.

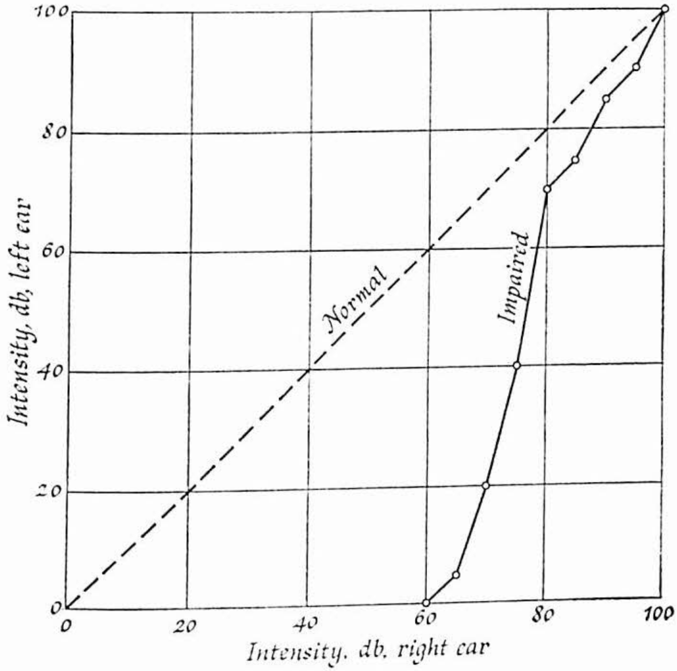

FIG. 1-19. Recruitment. The broken line shows how a normal person will make a binaural loudness match; the solid curve shows how a nerve-deaf ear 'catches up' with the normal ear at high intensities. Data from Fowler for a patient deafened to 2048 c/s in the right ear. (See Wever 1949.)

Experiments on the localization of sound. (1) The subject should sit in the centre of the room with his eyes covered. The experimenter makes various kinds of sound, each of short duration, from various positions round the subject, who indicates by pointing where he thinks each sound originates.

For demonstration purposes clicks may be produced with coins.

Tones may be produced with an oscillator (or audiometer) and an earphone which may be moved from place to place silently. The best system, perhaps, is to mount small 'speakers or 'phones round the subject and to switch the various tones or other sounds to these in random order.

(2) The ears may effectively be reversed with a *pseudophone*. This consists of two horns, one placed in each ear and bent round the head so that the sound reaches each ear from the opposite side of the head.

These two experiments may be combined. It is interesting to locate sounds with the pseudophone both with and without vision. The information from the auditory sense tends to be ignored when vision is available. This has been investigated by Jackson (1953) in an experiment in which spurious visual indications of sound sources were used. Held (1955) got his subjects to wear a pseudophone for several hours continuously and noted that this produced a systematic shift in sound localization.

Information in speech waves. A central and difficult problem is to discover which characteristics of the complex waves of speech are important for carrying information. These experiments take the form of destroying or modifying some features of the speech wave, and finding the effect upon intelligibility tests. One of the most striking findings is due to Licklider (1946), who showed that if the peaks of speech waves are cut off (this is quite simply done with two backed diodes) speech is still intelligible. Under conditions of noise masking, a small percentage of clipping may actually improve intelligibility. Important information is evidently carried by the time intervals of the cross-over points. It looks as though it is the ear's remarkable ability to discriminate very small time intervals – which was noted for binaural hearing when we discussed localization – which is vital to the recognition of speech.

The new concepts of Information Theory, first stated systematically by Shannon (1949), have proved of great use in quantifying the information transmitted in speech under various conditions. It is also important to the general problem of sensory discrimination. 'Information' is used in a special technical sense which does not involve meaning. 'Information' is defined in terms of the selection of items from a set of possible items. The larger the set the more difficult the choice, and the more 'information' is involved. The simplest possible case, and the case requiring least 'information', is a choice of one out of a set of two possible items. The unit of 'information' – the *bit* – is defined as a choice between two possibilities. Morse code is a very simple code containing only two alternative events or stimuli, a dot and a dash. It requires only a very simple receiving apparatus to distinguish between two possibilities, and mistakes are unlikely, but it requires a large number of these binary choices to represent a lot of information in a message. In a language containing a large number of characters (such as Chinese) a lot of information may be carried by each symbol, and then fewer symbols are needed for any given message. Thus the message will be shorter, but the apparatus required to discriminate the symbols from each other must be more complicated. Information is defined as *the logarithm of the number of alternatives*, the use of logarithms being convenient. Thus a choice of 1 out of 10 alternatives carries half the information carried by a choice of 1 out of 100 alternatives, and a third as much as 1 out of 1000 alternatives. The alternatives may be anything – playing cards, voltages, intensities, speech sounds, In practice logarithms to the base 2 are used. We may take an example: suppose that the ear can distinguish with equal ease (i.e. with equal probability of error) 16 things, no matter what these may be; this situation is characterized by 4 bits of information, since $\log_2 16 = 4$.

We should consider the case of a continuous scale, such as intensity or frequency. Suppose we have a measuring instrument, say a loudness or a frequency meter, graduated in divisions. Each division represents one of a number of possible values the variable might take. Now the greater the number of divisions on the instrument the greater the amount of information it can represent. To represent a continuous variable perfectly the instrument would need an infinite number of divisions, and its 'differential threshold' would have to be zero. It is thus in practice impossible to represent a continuous variable perfectly – it would require an infinite amount of information.

An important feature of all communication channels is 'noise'. This is any random disturbance of the signal. The effect of noise upon *channel capacity*, which is the *rate of transmission of information*, has been formulated in the Hartley-Shannon Law. If the band width (or frequency range) of the system is W, the mean signal power S, and the mean noise power N, then the Law states that the *channel capacity* is:

$$W = \log_2 (1 + \frac{S}{N}) \text{ bits/second}$$

It may be seen from this that by suitably 'trading' the variables it is possible to describe an infinite number of different systems having identical channel capacities, or information rates. Thus if there is plenty of time available, low band width and low signal/noise ratio may be adequate. But to transmit the same amount of information in a short time the band width and power must be increased, or the noise level must be reduced. Here the essential problem of sensory discrimination is formulated in a manner allowing quantified data to be obtained by experiment. Although the importance of discrimination has been stressed throughout, discussion of the formulation given by Shannon has been postponed until now, because speech is so familiar, and yet so baffling, that the formulation may seem of immediate use. Having thought this out, it might be worth the student's while to think again about the general problem of discrimination by the senses.

If a signal is masked by noise the moment to moment value of the noise will add to or subtract from the value of the signal energy. If the value of the signal plus the noise at this moment should represent some other piece of information, that is to say another symbol, then an error will occur unless the redundancy of the code is sufficient to allow rejection of the 'error signal'. Redundancy is the use of more symbols than would be necessary for the transmission of the message in a noise-free system. The larger the physical intervals between signals the less probable it is that noise will distort a symbol so that it is mistaken for another symbol. Thus to reduce the probability of errors: (*a*) the number of steps used as symbols should be small by comparison with the total range over which the receiver works, (*b*) the code should be redundant. Repetition is one form of redundancy. (*c*) Each symbol may be transmitted and received over a long time interval, the receiver incorporating some form of integrator. The signal/noise ratio increases with time, given integration. If the integrator is perfect, S/N increases with the $\sqrt{t}$, or with $\sqrt{N}$, where N is the number of repetitions of the message.

Many experiments have been done on recognizing words chosen at random from restricted vocabularies of various sizes under noise masking conditions. In general, intelligibility falls as the number of words in the vocabulary is increased. This is similar to the experiment in visual acuity mentioned in a previous section, in which the number of possible positions of the gap is changed. If either the number of words in the vocabulary or the number of speech symbols is increased, the chances of confusion and error through noise is increased.

We leave it to the reader to design an experiment involving the recognition of words in a noisy situation to test these assertions at least qualitatively. The quantative results of such experiments are of the greatest importance, but their study would take us too far from the sensory processes involved. The measures which may be obtained are, however, important for testing hypotheses of how central discrimination mechanisms might work, and in investigating the role of probability in the making of decisions.

The recognition of speech. The ear can distinguish tones best when they are about 50 db above threshold, and in the frequency range 500–4000 c/s (Miller 1951). The ear can thus handle, in the technical sense, most *information* in this region, because it can make most discriminations. There are about 1600 distinguishable tones and about 300 intensity levels (cf. Miller 1951, pp. 49–50). We cannot recognize so many, but they can be distinguished. The number of distinguishable pure sounds should thus be 300^{1600}. Comparatively few sounds are used as phonemes for speech, and there are fairly clear reasons for this. Individual voices are different; and yet all use the 'same' phonemes or set of phonemes; the speaker may be at various distances, and speak at various energy levels. The auditory system is capable of very much finer discrimination than is required for speech recognitions; this will tend to provide a safety factor against wrong identifications. The transmitted information is largely *redundant*, and this reduces errors.

The amount of information in a sound wave is given by

$$\text{Amount of information} = 2\text{TW} \log \frac{\text{S} + \text{M}}{\text{N}}$$

where $\log (s + n)/\text{N}$ can be regarded as the signal/noise ratio in decibels, W is the width of the frequency band, and T the duration of the message, or in words: *the amount of information in a speech wave is proportional to duration of the speech, to the range of frequency components involved, and to the logarithm of the number of discriminable steps in amplitude* (cf. Miller 1951).

In English only about 50 speech sounds (phonemes) are used out of all the possible combinations of frequencies and amplitudes. This is no doubt a concession to the noisiness of the world and of our nervous systems. Any masking noise will tend to reduce the information in the message; redundancy serves to preserve the message against noise or, to make this more explicit, to reduce the probability that any disturbance sufficiently important to cause an error will occur. In addition, language itself is redundant. In English 'q' is always followed by 'u', and so if 'u' were missing the word could still be read with certainty. Thus 'q een' can be read almost without hesitation. A gap such as 'b ead' can generally be filled in with a reasonable probability that no error will occur in a context. The same is true of spoken language, both in the physical characteristics of the speech sounds and in the statistical structure of the language.

REFERENCES

VISION

ADRIAN, E. D. (1928). *The Basis of Sensation. The Action of the Sense Organs.* London: Christophers.

ADRIAN, E. D. (1932). *The Mechanism of Nervous Action.* London: Oxford University Press

ANDERSON, E. E., and WEYMOUTH, F. W. (1923). 'Visual perception and the retinal mosaic'. *Amer. J. Physiol.*, **64**, 561.

BANISTER, H. (1932). 'Retinal action time'. *Phys. & Opt. Soc. Report on Vision.*

BARLOW, H. B. (1952). 'Eye movements during fixation'. *J. Physiol.*, **116**, 290.

BARLOW, H. B. (1953). 'Summation and inhibition in the frog's retina'. *J. Physiol.*, **199**, 69.

BARLOW, H. B. (1957). 'Increment thresholds at low intensities, considered as signal/noise discriminations'. *J. Physiol.*, **136**, 469.

BARLOW, H. B., FITZHUGH, R., and KUFFLER, S. W. (1957). 'Change of organisation in the receptive fields of the at's retina during dark adaptation'. *J. Physiol.*, **137**, 338.

BRINDLEY, G. S. (1952). 'The Bunsen-Roscoe Law for the human eye at very short durations'. *J. Physiol.*, **118**, 1, 135.

BRINDLEY, G. S. (1953). 'The effects on colour vision of adaptation to very bright lights'. *J. Physiol.*, **122**, 332.

BRINDLEY, G. S. (1957). 'Human colour vision'. *Progress in Biophysics*, Vol. **8**.

BROWN, J. F. (1931). 'The thresholds for visual movement'. *Psychol. Forsch.*, **14**, 249.

CAMPBELL, F. W. (1954). 'Accommodation reflex'. *Brit. Orthoptic J.*, **11**, 13.

CAMPBELL, F. W., and WHITESIDE, T. C. D. (1950). 'Induced pupillary oscillations'. *Brit. J. Ophthal*, **34**, 180.

CARPENTER, A. (1948). 'The rate of blinking during prolonged visual search'. *J. exp. Psychol.*, **38**, 587.

COGGAN, D. G. (1956). *Neurology of the Ocular Muscles*. London: Thomas.

COOPER, S., DANIEL, P. M., and WHITTERIDGE, D. (1953). 'Nerve impulses in the brain stem of the goat. Short latencies obtained by stretching the extrinsic eye muscles and the jaw muscles'. *J. Physiol.*, **120**, 471.

CRAIK, K. J. W. (1938). 'The effect of adaptation on differential brightness discrimination'. *J. Physiol.*, **92**, 406.

CRAIK, K. J. W. (1939). 'The effect of adaptation upon visual acuity'. *Brit. J. Psychol.*, **29**, 252.

CRAIK, K. J. W. (1947). 'Theory of the human operator in control systems. I. The operator as an Engineering system'. *Brit. J. Psychol.*, **38**, 56.

CRAIK, K. J. W., and ZANGWILL, O. L. (1939). 'Observations relating to the threshold of a small figure, within a contour of a closed-line figure'. *Brit. J. Psychol.*, **30**, 149.

DODGE, R. (1907). 'An experimental study of visual fixation'. *Psychol. Monog.*, **8**, No. 4.

DREW, G. C. (1950). 'Variation in reflex blink rate during visual-motor tasks'. *Quart. J. exp. Psychol.*, **3**, 73.

DUNCKER, K. (1939). 'Induced motion', in *Source Book of Gestalt Psychology*, ed. W. E. Ellis. New York: Harcourt.

FRY, G. A., and BARTLEY, S. H. (1935). 'The effect of one border in the visual field upon the threshold of another'. *Amer. J. Physiol.*, **112**, 414.

FULTON, J. F. (1955). *A Text Book of Physiology* (17th ed.). London: Saunders.

GRAHAM, C. H., and MARGARIA, R. (1935). 'Area and the intensity-time relation in the peripheral retina'. *Amer. J. Physiol.*, **113**, 299.

GRAND, LE Y. (1957). *Light, Colour and Vision.* London: Chapman & Hall.

GRANIT, R. (1947). *Sensory Mechanisms of the Retina.* London: Oxford University Press.

GREGORY, R. L. (1952). 'Variations in blink rate during non-visual tasks'. *Quart. J. exp. Psychol.*, **4**, 165.

GREGORY, R. L. (1955a). 'A note on summation time of the eye as indicated by signal/noise discrimination'. *Quart. J. exp. Psychol.* **7**, 147.

GREGORY, R. L. (1955b). 'Colour anomaly, the rayleigh equation and selective adaptation'. *Nature*, **176**, 172.

GREGORY, R. L. (1956). 'An experimental treatment of vision as an information source and noisy channel'. *Third London Information Symposium*, ed. C. Cherry. London: Butterworths.

GREGORY, R. L. (1958). 'Eye movements and the stability of the visual world'. *Nature*, **182**, 1214.

GREGORY, R. L. (1959). 'A blue filter technique for detecting eye movements during the autokinetic effect'. *Quart. J. exp. Psychol.*

GREGORY, R. L., and CANE, V. R. (1955). 'A statistical information theory of visual thresholds. *Nature*, **176**, 1272.

GREGORY, R. L., and ZANGWILL, O. L., 'The autokinetic effect'. *Quart. J. exp. Psychol.* (in the press).

GUILFORD, J. P., and DALLENBACH, K. M. (1928). 'A study of the autokinetic sensation'. *Amer. J. Psychol.*, **40**, 83.

HARTLINE, H. K. (1934). 'Intensity and duration in the excitation of single photoreceptor units'. *J. Cell. Comp. Physiol.*

HARTLINE, H. K. (1938). 'The response of single optic nerve fibres of the vertebrate eye to illumination of the retina'. *Amer. J. Physiol.*, **121**, 400.

HARTLINE, H. K. (1940). *J. Opt. Soc. Amer.*, **30**, 239.

HECHT, S. (1934). 'The nature of the photoreceptive process', in *A Handbook of General Experimental Psychology*, ed. C. Murchison. Worcester Clark University Press.

HECHT, S., and MINTZ, E. U. (1939). 'Divisibility of single lines at various illuminations and the retinal basis of visual resolution'. *J. gen. Physiol.*, **22**, 593.

HECHT, S., SCHLAER, S., and PIRENNE, M. H. (1942). 'Energy quanta and vision'. *J. gen. Physiol.*, **25**, 819.

HELMHOLTZ, H. VON (1852). *Physiological Optics*, trans. J. P. C. Southall (American Optical Society, 1924).

HICK, W. E. (1949). 'Reaction time for the amendment of a response'. Unpublished Report No. 93, A.P.U. Cambridge.

HOLMES, G. (1938). 'The cerebral integration of ocular movements'. *Brit. Med. Journ.*, **II**, 107.

HOLST, E. VON (1954). 'Relations between the central nervous system and the peripheral organs'. *Brit. J. anim. Behav.*, **2**, 89.

KOFFKA, K. (1935). *Principles of Gestalt Psychology*. New York: Harcourt Brace.

KORNMÜLLER, A. E. (1930). 'Eine Experimentelle Anasthesie der ausseren Augenmuskeln am Menschen und Ihre Auswirkungen'. *J.f. Psychol. und Neurol.*, **41**, 354.

KUFFLER, S. W. (1953). 'Discharge of patterns and functional organisation of mammalian retina'. *J. Neurophysiol.*, **16**, 37.

LASHLEY, K. S. (1941). 'Patterns of cerebral integration indicated by the scotomas of migraine'. *Arch. Neurol. Psychol.*, **46**, 331.

LEIBOWITZ, H. W. (1955). 'The relation between the rate threshold for perception of movement for various durations of exposure'. *J. exp. Psychol.*, **49**, 209.

MORGAN, C. T., and STELLAR, E. (1950). *Physiological Psychology*. New York: McGraw-Hill.

OSTERBERG, G. (1935). 'Topography of the layer of rods and cones in the human retina'. *Acta Ophthal. Suppl.*, **61**, 1.

PIRENNE, M. H. (1948). *Vision and the Eye*. London: Chapman & Hall.

PIRENNE, M. H. (1956). 'Physiological mechanisms of vision and the quantum nature of light'. Biological Reviews of the Cambridge Philosophical Society.

PIRENNE, M. H., and DENTON, E. S. (1952). 'Accuracy and sensitivity of the human eye'. *Nature*, **170,** 1039.

POLYAK, S. I. (1941). *The Retina*. Chicago: University of Chicago Press.

PONDER, E., and KENNEDY, W. P. (1927). 'On the act of blinking'. *Quart. J. exp. Physiol.*, 1927.

POULTON, E. C., and GREGORY, R. L. (1952). 'Blinking during visual tracking'. *Quart. J. exp. Psychol.*, **4,** 57.

PULFRICH, C. (1922). *Naturwissenschaften*, **10,** 553.

RAWDON-SMITH, A. F. (1938). *Theories of Sensation*. Cambridge University Press.

RUSHTON, W. A. H. (1953). *Aspects of Retinal Physiology*. Instituto de Biofisica, Universitade do Brasil.

RUSHTON, W. A. H., and CAMPBELL, F. W. (1954). 'Measurement of rhodopsin in the living human eye'. *Nature*, **174,** 1096.

RUSHTON, W. A. H., CAMPBELL, F. W., HAGINS, W. A., and BRINDLEY, G. S. (1955). 'The bleaching and regeneration of rhodopsin in the living eye of the albino rabbit and of man'. *Optica*, Acta **1,** 183.

SHANNON, C. E., and WEAVER, W. (1949). *The Mathematical Theory of Communication*. Urbana: University of Illinois Press.

SLOAN, L. L., and ALTMAN, A. (1954). 'Factors involved in several tests of binocular depth perception'. *M. A. Arch. Ophthalmology*, **52,** 524.

STARK, L., CAMPBELL, F. W., and ATWOOD, J. (1958). 'Pupil unrest: an example of a biological servomechanism'. *Nature*, **182,** 857.

TRAVIS, R. C. (1936). 'The latency and velocity of the eye in saccadic movements'. *Psychol. Monog.*, **47,** No. 2, 242.

VINCE, M. (1948). 'The intermittency of control movements and psychological refractory period'. *Brit. J. Psychol.*, **38,** 149.

WALLS, G. L. (1942). *The Vertebrate Eye and its Adaptive Radiation*. Cranbrook Institute of Science.

WALSH, E. G. (1957). *Physiology of the Nervous System*. London: Longmans.

WERTHEIMER, MAX (1912). *Zschr. Psychol.*, **61.**

WILLMER, E. N. (1946). *Retinal Structure and Colour Vision: a Restatement and an Hypothesis*. Cambridge University Press.

WOHLGEMUTH, A. (1911). 'On the after-effect of seen movement'. *Brit. J. Psychol. Monograph Supplement.*

WRIGHT, W. D. (1936). 'The breakdown of colour match with high intensities of adaptation'. *J. Physiol.*, **87**, 23.

WRIGHT, W. D. (1946). *Researches on Normal and Defective Colour-Vision.* London: H. Kimpton.

YOUNG, T. (1807). *A Course of Lectures on Natural Philosophy.* London: Johnson.

Recommended general books

BRAGG, W. (1933). *The Universe of Light.* London: G. Bell.

HELMHOLTZ, H. VON (1852). *Physiological Optics*, trans. J. P. C. Southall (American Optical Society, 1924).

PIRENNE, M. H. (1948). *Vision and the Eye.* London: Chapman & Hall.

VERNON, M. D. (1937). *Visual Perception.* Cambridge University Press.

VERNON, M. D. (1952). *A Further Study of Visual Perception.* Cambridge University Press.

WRIGHT, W. D. (1946). *Researches on Normal and Defective Colour Vision.* London: Henry Kimpton.

HEARING

BÉKÉSY, G. VON (1956). 'Simplified model to demonstrate the energy flow and formation of travelling waves similar to those found in the cochlea'. *Proc. Nat. Academy of Science*, **42**, 12, 930.

BÉKÉSY, G. VON (1957a). 'The ear'. *Scientific American.*

BÉKÉSY, G. VON (1957b). 'Sensations on the skin similar to directional hearing, beats, and the harmonics of the ear'. *J. Acoust. Soc. Amer.*, **29**, 4, 489.

CULLER, E. A., COAKLEY, J. D., LOWY, K., and GROSS, N. A. (1943). 'A revised frequency-map of the guinea-pig cochlea'. *Amer. J. Psychol.*, **56**, 475.

DAVIES, H. (1957). 'Biophysics and physiology of the inner ear'. *Physiol. Rev.*, **37**, 1, 1.

DIX, M. R., HALLPIKE, C. S., and HOOD, J. D. (1948). 'Observations upon the loudness recruitment phenomenon', *Proc. Roy. Soc. Med.*, **41**, 516.

EGAN, J. P., CLARKE, F. R. and CARTERETTE, E. (1956). *J. Acoust. Soc. Amer.*, **28**, 4, 536.

FLETCHER, H. (1929). *Speech and Hearing*. New York: Von Nostrand.

FLETCHER, H. (1938). 'The mechanisms of hearing as revealed through experiment on the masking effect of thermal noise'. *Proc. Nat. Acad. Sci. Washington*, **24**, 265.

GREGORY, R. L. (1958). 'Increase in "neural noise" as a factor in ageing', in *Proc. Fourth International Congress of Gerontology*.

GREGORY, R. L., and WALLACE, J. G. (1958). 'A theory of nerve deafness'. *The Lancet*, Jan. 11, 1958, 83.

HELD, R. (1955). 'Shifts in binaural localisation after prolonged exposures to atypical combinations of stimuli'. *Amer. J. Psychol.*, **68**, 526.

HOOD, J. D. (1950). 'Studies in auditory fatigue and adaptation'. *Acta Oto-Laryngolica Supplementum*, **XCII.**

JACKSON, C. V. (1953). 'Visual factors in auditory localisation'. *Quart. J. exp. Psychol.*, **5**, 52.

LICKLIDER, J. C. R. (1946). 'Effects of amplitude distortion upon the intelligibility of speech'. *J. Acoust. Soc. Amer.*, **18**, 429.

MARLER, P. (1955). 'Characteristics of some animal calls'. *Nature*, **176**, 6.

MILLER, G. A. (1951). *Language and Communication*. New York: McGraw-Hill.

SHANNON, C. E. (1949). 'A mathematical theory of communication'. *Bell System Technical Journal*, **27**, p. 379 and p. 623. See Shannon, C. E., and Weaver, W. (1950).

TANNER, W. P., Jr., and SWETS, J. A. (1954). 'A decision-making theory of visual detection'. *Psychol. Rev.*, **61**, 401.

WALLACH, H., NEWMAN, E. B., and ROSENZWEIG, M. R. (1949). 'The precedence effect in sound localisation'. *Amer. J. Psychol.*, **62**, 315.

WELFORD, A. T. (1958). *Ageing and Human Skill*. Oxford University Press.

WEVER, E. G. (1949). *Theory of Hearing*. New York: John Wiley.

WEVER, E. G., and LAWRENCE, M. (1954). *Physiological Acoustics*. Princeton University Press.

General references

BROADBENT, D. E. (1958). *Perception and Communication.* Pergamon Press.

CHERRY, C. (1957). *On Human Communication.* New York, London: John Wiley.

WEVER, E. G. (1949). *Theory of Hearing.* New York, London: John Wiley.

WEVER, E. G., and LAWRENCE, M. (1954). *Physiological Acoustics* Princeton University Press.

Chapter 2

PERCEPTION

N. F. DIXON

University College, London

First and foremost perception is the process whereby the individual experiences an environment. Beyond this the perceptual response has been regarded as, if not a necessary, at least a desirable prelude to those subsequent actions, responses or behaviour patterns whereby an organism so adjusts itself, or its environment, as to maximize its own chances of survival. According to this viewpoint, perception is essentially adaptive, the first stage in regulative behaviour, the input side of a feedback device whereby the external world including the body of the percipient is brought into line with the needs of the individual, both social and biological. Then again perception has been described as an activity of mind, the elaboration of subjective phenomena which ordinarily, though not necessarily, owes something to the physical stimuli that activate the sense receptors.

If this is the nature of the perceptual process, what of its product?

Here, irrespective of their ultimate function it is convenient to regard percepts as lying along a number of different continua. Of these, the most important is that of veridicality, or consonance between their content and the external world. Thus hallucinations, dreams, illusions, so-called misperceptions and incomplete perceptions are merely points towards one extreme of such a continuum, one which ranges from the truly fantastic and idiosyncratic at one end, to the photographically truthful and complete picture of reality at the other. Now it is in the nature of the perceptual response that percepts at the latter end of this continuum are as rare, if not rarer, than those at the former. For perception is less concerned with the wholely accurate and complete representation in consciousness of what is outside the individual than with fashioning a visual, auditory, olfactory, tactual and gustatory world that is at once both 'true' *and* appropriate at the moment of perceiving. To perceive all would be nearly as unadaptive as to perceive nothing. In the same vein, to perceive the same situation in the same way at all times and as all others perceive it would be hardly more useful than to hallucinate. For far from being a passive representation of what is there, perception shows itself to be a highly selective 'effort after meaning' (Bartlett 1932) whereby the individual brings to bear upon the information available at his sense organs a cognitive structure determined by his needs, attitudes, previous experience and biological make up. An extreme version of this view is that advanced by the 'transactionalists'. For this school of thought (Ittelson 1960), perception is 'that part of the process of living by which the individual, from his own particular point of view, creates for himself the world within which he has his life's experiences and through which he strives to gain his satisfactions'. Beyond this even there is a sense in which perception is something *sui generis* resulting from cerebral activity which may persist even in the absence of afferent stimuli. This has been exemplified by recent work on the effects of sensory deprivation (Solomon *et al.* 1957). These studies have made it clear that left to its own devices and insulated from external information, the brain will continue to elaborate percepts albeit of little value to the individual. One importance of this finding is that it justifies yet another view of *normal* perception, namely that it be regarded as the process whereby external stimuli control, direct, and shape the content of consciousness.

However, whatever one's theoretical viewpoint, the problems of perception remain the same. There are, on the one hand, those of explaining the discrepancies between what is perceived and what is there; on the other, those of investigating such factors in the stimulus and in the percipient which determine the precise nature of particular perceptual phenomena.

As to the first, three sorts of explanation have been given for the discrepancies encountered in trying to relate what is perceived to the raw sensory data received. Gestaltists in the tradition of continental philosophy incline to the view that while percepts owe something to the external stimuli, they also reflect the effect of certain organizing principles 'built in' to the nervous system of the organism.

There are two questions here for experimental research – that of demonstrating the operation of *praegnanz** and of certain other principles in the perception of normal persons; and the far more difficult one of testing the hypothesis that these principles are unlearnt, that they are in fact the psychological correlates of certain intrinsic attributes of the human nervous system.

A second explanation in the English empiricist tradition is that perception is largely learned – that the percept is the resultant of the physical stimulus and the past experience of the percipient. This is the view subscribed to by Vernon (1952) when she says 'a percept . . . exists as a part of a systematic categorization of experience in concepts and schemata'.

It is a view with a great deal of experimental backing. Exactly how and to what extent experience does determine perception are problems which will be investigated by the experiments to be described.

Finally, and in recent years, the view has been put forward that perception can only be understood in the context of certain personality variables. This theory proposes that what we perceive depends very largely upon such factors as present needs, emotional state, attitude, prejudice and the like. It is a theory that makes contact with personality dynamics on the one hand and psychophysics on the other.

Now in the history of the psychology of perception countless experiments have been performed which support one or the other of these three sorts of explanation for certain perceptual phenomena. Thus Gestalt psychologists have concentrated in the main in arranging for investigation such perceptual situations as will maximize the probability of explanation in terms of the nativistic organizing tendencies and field theory. The 'New Lookers' on the other hand have devised experiments with an emphasis on the emotive factors in perception, experiments in which the independent variables are either the need-relatedness of the stimulus presentation or the personality make-up of the individual, or his present need state.

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* The original statement was given by Köhler in 1920. A free translation is: *In all processes which, in general, lead to an end-state independent of the time* (i.e. to an equilibrium), *the displacements tend in the direction of a minimum of structural energy* (*Phys. Gestalt*, 248). The statement is meant to apply to both physical and psychophysical systems, although the reasoning is physical, the purpose of the book being to show the parallel between physical and psychophysical events.

Purpose of experiments. For our present purpose, however, it is proposed to adopt an eclectic view and present three experiments each of which relates in some measure to all three types of perceptual theory. From each of these experiments conclusions may be drawn regarding (*a*) the role of organizational factors in perception, (*b*) the influence of past experience and cognition upon perception, and (*c*) the effect of personality and emotive factors upon what is perceived in a given situation.

THE PHI PHENOMENON

Introduction. The phi phenomenon refers to an illusion of movement between successive presentations of two momentarily exposed, spatially separated, stationary objects. Under certain critical conditions of intensity and of time and distance they appear as one object moving from the position occupied by the first object to that occupied by the second. Writing about this phenomenon in her *Further Study of Visual Perception* (1952) M. D. Vernon comments that the amount of experimental work devoted to the study of apparent movement is out of all proportion to its importance in the psychology of perception. Certainly a great number of experiments have been devoted to a study of the phi phenomenon and it is undoubtedly true, as Vernon points out, that the perceptual experience of apparent movements is a comparatively rare and unimportant event in normal everyday life. However, the writer maintains that, for theoretical and other reasons, the phi phenomenon *is* of the greatest importance for the student of perception. To start with it is of undoubted historical interest as that perceptual phenomenon which stimulated Wertheimer (1912), and through him later gestaltists (Koffka 1935; Köhler 1929), to develop what was at the time a much-needed new way of looking at perception.

Whatever reservations contemporary psychologists may have about such aspects of Gestalt theory as the hypothesis of cortical isomorphism, there can be little doubt that this theory, in contrast with an earlier elementarist-associationist set of postulates, contributed little short of a Copernican shift in the history of present day psychology. It was as important for the development of perceptual theory as was Kant's (1781) theory of knowledge (which in many respects it parallels) for present day philosophy.

These are somewhat sweeping assertions. How in the context of the phi phenomenon can they be supported in detail? In the first place being an illusion the phenomenon provides unassailable evidence for refutation of the old and naïve 'constancy' hypothesis, the hypothesis that we perceive what is there, the hypothesis that assumes a photographic verisimilitude in perception. In the second place, the study of phi and kindred illusions necessitated the adoption of a new attitude towards the sort of processes that must underlie the perceptual response. This new approach so admirably expressed in Köhler's (1939) discussion on dynamism versus mechanism had repercussions of a revitalizing kind throughout all areas of psychology. One does not have to subscribe to the notion of built-in organizing processes or of the distribution of field forces in the brain to realize the extent to which Gestalt theory stimulated all manner of interesting research and helped to bridge the gap between psychology and physiology.

Finally, as it is hoped to demonstrate in the following experiments, the phenomenon provides a perceptual experience of great sensitivity to the influence of many and varied independent variables which the experimenter might be concerned with studying. Some of these are exemplified in the following cross-section of researches involving investigation of phi in different contexts. For a more exhaustive treatment of the subject, the reader is referred to the previously cited book by Vernon.

The earliest experiments by Wertheimer (1912) were primarily concerned with investigating the effect upon phi of manipulating the variables of time and distance intervals *between* stimuli, and of duration and intensity *of* the stimuli. The results from these experiments showed that for the occurrence of phi a given relationship must obtain between these variables. This finding has been expressed by Korte (1919) in the form of three 'laws' which state that for optimal phi there is a direct relationship between the time and distance intervals, an inverse relationship between time interval and intensity of the stimuli, and a direct relationship between distance and intensity. Though only true to a limited extent (Cermak 1922) these conclusions were important to Gestalt theory in so far as they are compatible with the idea of radiating forces between local stimuli in the brain field; phi, according to the principle of isomorphism, being the perceptual experience that results from a meeting of two such radiations between their points of origin but nearer the second than the first.

No less important for Gestalt theory were later experiments by Dimmick and Scahill (1925) and Brown and Voth (1937). These studies supported the Gestalt contention that real movement depended upon just such 'built-in principles of cortical organization' as were responsible for apparent movement. Their method was simply to show an inability on the part of normal subjects to distinguish between phi and real movement. While perceptual similarity is of course no proof of an identical underlying mechanism these studies did lend credence to the Gestalt viewpoint, especially since they provided subsidiary evidence for field effects; this evidence was provided by the shrinkage of the circular path described, either by a single rotating light source, or the apparent movement produced by a circle of lights flashing in sequence. There remained however at least one alternative to the Gestalt explanation in terms of cortical conduction. It was that the phenomenon occurred through eye movements. This hypothesis, however, appeared, to say the least, unlikely in the light of experiments by Wertheimer and Gengerelli (1948) which demonstrated the possibility of simultaneous phi in two directions at once.

While the capacity for perceiving phi is no doubt rooted in the inherent dynamics of the nervous system the phenomenon is none the less subject to the effects of learning and past experience, as is shown by the fact that its strength is strongly affected by the meaningfulness of the context in which it occurs. Thus it can be shown that the apparent arm movements of a 'stick man' are more pronounced than those of two straight lines presented under otherwise similar conditions. Apropos of this, Jones and Bruner (1954) from a series of experiments investigating phi in various contexts, both meaningful and meaningless, conclude that 'out of familiarity comes a matrix of expectancies ordering and interrelating the cue to apparent movement . . . the organism learns to expect movement in some contexts more than in others and learns to expect certain *kinds* of movement from some objects and not from others'.

Finally in this brief review of experimental work involving the phi phenomenon there are studies in which it has featured as an independent variable in the investigation of quite other parameters of the individual.

They are exemplified in two experiments by Christman (1953) and Livson (1953) respectively investigating the effect of prior experience of apparent movement on, in the one case, figural aftereffects and, in the other, the autokinetic phenomenon; experiments which were deemed to provide some support for the Köhler-Wallach Satiation hypothesis, and the general theory of cortical isomorphism.

Of a rather different kind are experiments in which phi has been used in the context of inter-retinal and inter-hemisphere interaction. In support of a 'central' theory phi was found to occur whether the two stimuli were applied monocularly or binocularly, whether one was presented to each half of the single retina, or both in the same half, or whether the two were presented separately to the two eyes in different or the same visual field.

Yet other studies (Bender and Teuber 1949; Brenner 1956) have been concerned with the effect upon phi of brain damage, the main findings here being that, for people so afflicted, real and apparent movement might be replaced by the perception of a succession of stationary objects, or in other cases by curvilinear motion.

Finally, in this overview the ambiguous nature of phi as a perceptual experience has occasioned its use in yet another experimental context, that of perceptual typologies. In one such study, Hamilton (1960), the tendency to perceive phi under appropriate stimulus conditions was shown to depend upon the age, personality type and neuroticism of the individual participant. Thus older people were found less able to see it than younger ones and those with lowest tolerance for ambiguity less so than normals. An important feature of this research was that its results were consistent with the general concept of ambiguity tolerance (Frenkel-Brunswik 1949) for it showed that those persons with the strongest defences against internal conflict, i.e. obsessionals and conversion hysterics, were those least able to perceive the reality-conflicting phenomenon of phi. The value of this experiment and others like it is not that they inquire into the nature of a perceptual phenomenon *per se* but rather that they are concerned with individual differences, and, in the present instance, provide support for the view expressed by Klein that individuals have a generalized perceptual attitude expressing certain generalized principles of cognitive control.

So much for an example of the many and varied contexts in which phi has been studied. In some respects the following short experiments for classroom work encompass much that has been covered in this introduction. The necessary compression, and restriction in data, required by this approach, is excused on the grounds that, for the student of perception, adequate demonstration and familiarity with techniques should precede the sheer collection of data.

In general the experiments are concerned, respectively, with testing Korte's laws, investigating the effect of receptor area stimulated, examining the effect of meaning upon phi, and finally with a comparison between real and apparent movement. Most of these small experiments can be administered to a group if necessary rather than to individual subjects. In either case, however, one version of each piece of apparatus to be described should prove sufficient for a laboratory class of twelve students working, say, in groups of three over a period of three hours.

EXPERIMENT Ia

Hypotheses. For the occurrence of phi:—1. With intensity held constant the distance between the stimuli is directly proportional to the time interval between their presentations.

2. For any given distance there is an inverse relationship between the length of the time interval and the intensity of the stimuli.

3. With time held constant there is a direct relationship between stimulus intensity and the distance over which phi can be seen to occur.

Apparatus. For investigating Korte's laws the following apparatus designed by the author has been found quite sufficient.

A_1 and A_2 are two wooden, metal or hardboard boxes 6 × 6 × 6 inches painted black on the outside and white internally. At the back of each box there is a gas discharge tube, L_1 and L_2 respectively. The front of each box contains an aperture 3 × 3 inches behind which is fixed a rectangle of opal flashed glass. On the outside of each aperture is a slide holder H_1 and H_2. The fronts of the two boxes are joined by

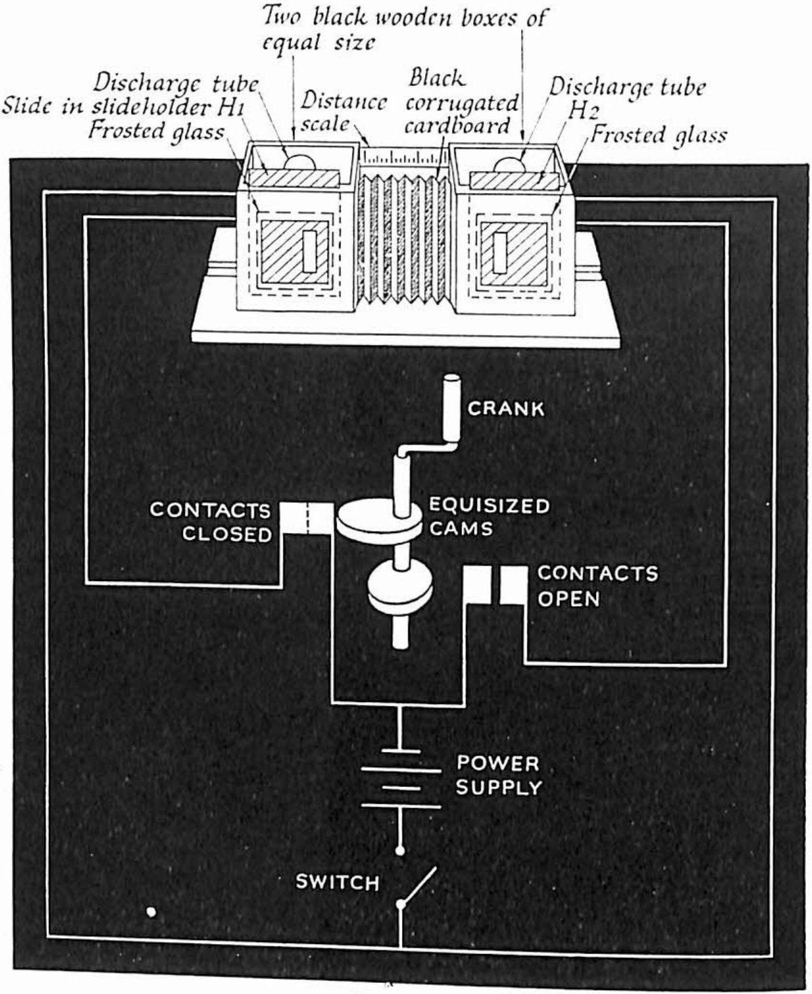

FIG. 2–1.

a piece of heavy black corrugated cardboard behind which is a screw and lever for varying the distance between the boxes. Timing is achieved by micro switches (in the lamp circuits) that are triggered by a pair of cams hand driven via the crank C. Stimulus intensity can be varied by inserting filters or paper between the opal glass screens and the slides. The slides consist of metal or fibre squares in which slots are cut off-centre. By this means both distance between and orientation of the stimuli can be varied.

Some general considerations as to method. To test the first hypothesis for example, the independent variable will be the time interval between stimulus presentations, and the dependent variable that distance between stimuli at which the subject reports the best impression of phi. In considering the method attention should be paid to the following points. What time intervals should be employed? Should they be varied systematically (*the method of limits*), or presented randomly (*the constant method*). For any given time interval how should the distance be varied, progressively increased or decreased by the experimenter, varied randomly, or varied by the subject (*the method of adjustment*)? Whatever the method chosen, what instructions will be given to the subject? Finally what other factors need to be controlled if such results as are obtained can be ascribed to the effect of time upon distance. Taking the last point first it is a useful discipline to imagine some hypothetical results that might be obtained and then formulate explanations for such results that are alternative to the hypothesis under test. These explanations will suggest the factors that should be controlled or held constant during the experiment. They will include such things as general level of illumination, state of adaptation of the subject, the instructions given, the distance between the stimulus and the subject's eyes, and the orientation of his head. All in all these considerations suggest that it will be essential for the experimenter and his assistant to carry out a brief pilot experiment to familiarize themselves with the apparatus and to establish time interval and distance ranges over which phi might be expected to occur. They should also decide upon a way of measuring the time intervals. Two possibilities present themselves. In one the experimenter moves the cam lever back and forth in time to a metronome previously calibrated in seconds and fractions of a second. In the other the cam wheel is rotated at a given speed as measured by stopwatch.

Notwithstanding these various considerations and the advisability of the student working out his own method the following procedure exemplifies one way in which the experiment might be conducted.

Hypothesis 1. Procedure. S is seated in a semi-darkened room with his head on a chin rest so that his eyes are on a level with and one yard from the two stimulus boxes. After five minutes for dark adaptation (longer should be allowed when time permits) he is given the following instructions: 'I am going to show you two vertical lines of light in quick succession (demonstrate by operating cam lever). Depending upon how quickly they succeed each other and how great is the distance between them, you will see them as either two lines of light presented simultaneously, two lines of light appearing one after the other, or one line of light which moves back and forth from left to right and vice versa.

'All you are required to do is to signify which of these three things you see each time I ask you. Any questions?'

At each of four time intervals presented in a random order the distance between the stimuli is progressively increased and then progressively decreased. During each such trial the subject is required to signal what he sees according to the instructions given.

Results. A record of the subject's responses will enable entries to be made in the following table:

TABLE I. Mean distance between stimuli in inches

Time intervals:	*50 m.s.*	*100 m.s.*	*150 m.s.*	*200 m.s.*
Phenomenon reported:				
(*a*) Two simultaneous lines				
(*b*) Phi				
(*c*) Two successive lines				

The distance over which phi is perceived should then be plotted as a function of the time interval between stimulus presentations.

Hypothesis 2. Procedure. For investigating the relationship between time and intensity it is suggested that the method of adjustment be used. The procedure is exemplified below.

Under the same general experimental conditions as applied in procedure 1 the operation of the cam timer is explained to the subject who is then given the following instructions: 'Whenever I give you the signal "now" I want you to turn the cam time handle at such a speed as to produce the greatest impression of movement between the two positions of the stimulus light. All you have to do is to rotate the cam at various speeds until you find that which maximizes the impression of movement. When you have found the best speed maintain it until I tell you to stop.'

With the distance between the stimuli held constant, the experimenter then times the subject's rotation speed under four levels of stimulus intensity. Differing levels may be obtained by using standard neutral filters or, failing this, varying thicknesses of typing paper between the lamp and slide.

The order of application of the different intensity levels should be randomized differently for each subject.

Results. From the speed of rotation at which S experiences phi the actual time intervals should be computed, and entries made in the following table:

TABLE II. Time intervals at which phi reported

Intensity level:	*1*	*2*	*3*	*4*
Trial 1				
2				
3				
etc.				
Means				

As with hypothesis 1 the experimenter should also present the results graphically plotting time against intensity.

Hypothesis 3. To investigate the relationship between distance and intensity the procedure is broadly the same as for the test of hypothesis 1. The main difference is that the experimenter now varies stimulus intensity while holding the time interval constant. Regarding the latter it is suggested that that interval which was found to produce the strongest impression of phi be the one used. Stimulus intensity should be varied as in the previous experiments.

Results. Data from this study should enable entries to be made in Table III.

General conclusions from individual results. The purpose of this experiment was to examine the validity of Korte's laws. The results, including introspective data, from each of the two experiments should be examined in this context and conclusions drawn regarding the truth of the laws.

TABLE III. Distances at which phi reported

	Intensity 1	*Intensity 2*	*Intensity 3*	*Intensity 4*
Trial 1				
2				
3				
4				
Means				

Class or group results. Provided the various experimenters have followed identical procedures it should be possible to prepare three tables showing the relationships between intensity, time and distance for the group. The values in these tables will of course be the mean scores for each individual.

The following tests of significance may be applied to the data:

1. 't' tests in which the mean distances and times at which subjects report phi are compared under differing time, distance and intensity conditions.

2. Correlations; in which the coefficient of concordance between subjects in the degree of correlation they show between time and distance, distance and intensity, and time and intensity is computed by the method described by Kendall (1948).

In addition to these statistics, curves should be plotted for the group means just as they were for the individual means.

Main discussion and conclusions. In discussing the results and drawing conclusions the following points should be covered. First the results should be viewed in the light of Korte's laws. Secondly their relevance to the Gestalt theory of isomorphism needs to be examined. Finally there should be some discussion of any anomalies in the results, particularly with a view to suggestions for further research in this area.

EXPERIMENT Ib

This experiment is concerned with investigating the effect upon phi of the receptor areas stimulated. In particular it is designed to compare, on the one hand, intra with inter retinal stimulation, and, on the other, the effect of stimulation that involves one cerebral hemisphere with that which involves both hemispheres. The following hypotheses particularize the investigations to be carried out.

The tendency to perceive phi will be greatest when (*a*) stimulation is binocular rather than monocular, (*b*) both stimuli are presented to one eye rather than when one is presented to one eye and the other to the other, *(*c*) when the two stimuli are presented in the same rather than different visual fields.

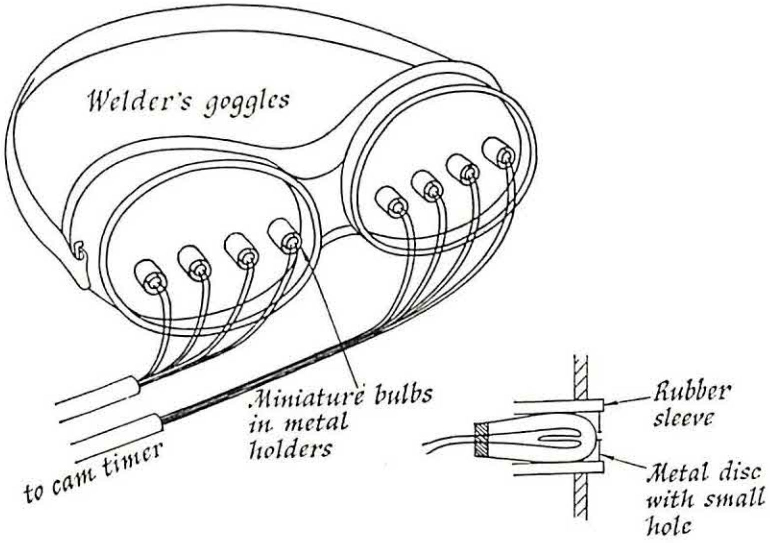

FIG. 2–2.

Apparatus. The apparatus consists of a pair of goggles in which the glass has been replaced by metal or fibre drilled to take midget light bulbs that are wired back to the cam timer described earlier. By having a number of holes drilled in each eyepiece it is possible to vary within limits the regions of each retina stimulated.

Method. The main problem of method in this experiment lies in measurement of the dependent variable. No longer is it a question of recording merely the presence or absence of phi. Rather do we have to find a measure which will be sensitive to qualitative changes in phi under different conditions of stimulation. Three possibilities present themselves – free verbal report, a verbal scale, some other measurable non-verbal index of phi. For our present purposes it is proposed to use a combination of all three. That is, for each presentation made the subject will be asked to (*a*) describe the perceptual experience, (*b*) make a comparison between it and preceding perceptual experiences, and (*c*) demonstrate the time interval range over which phi still occurs. Implicit in this last measure is the idea of defining strength of phi in terms of its resistance to changes in the interval between stimulus presentations.

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* When time is very limited it is suggested that the testing of this hypothesis be omitted. Its theoretical importance and method of testing should of course still be given consideration.

Inasmuch as the independent variable is the receptor area stimulated the experiment will involve a sequence of switching operations by the experimenter. This sequence should be prepared beforehand and so balanced as to reduce any order effect.

Procedure. Assuming that the subject's eyes have been tested for normality and approximate equality in resolving power and sensitivity, he is tested in a reclining position, and the goggles adjusted so as to exclude all light apart from that provided by the pea bulbs.

By switching on pairs of lamps in turn the experimenter then finds two pairs of lamps, where one lamp of each pair is presented to one eye and the other to the other, such that each pair is seen as a single point of light. Two typical such corresponding pairs AB + CD are shown in the following diagram.

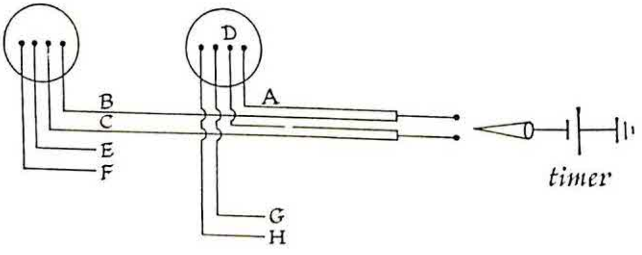


FIG. 2–3.

The subject is then instructed to rotate the cam timer at the slowest speed at which he perceives apparent movement between the pair AB and the pair CD. The rate of rotation and hence time interval is measured as in the previous experiment. The subject is then required to rotate the cam at the fastest speed at which he perceives phi. The time interval between the illumination of AB and that of CD is computed as before.

Having completed the first trial involving binocular stimulation in like fields the procedure is repeated for the single lamps A and C; B and D being now disconnected. Again the subject indicates the time interval range over which the phenomenon occurs. He is also asked to describe the perceptual experience and to indicate whether phi is now more pronounced, less pronounced, or equal to that experienced on the previous trial.

This general procedure is then repeated for all possible pairs of lamps relevant to the hypotheses under test. Including those already referred to there will be:

	1st Stimulus	*2nd Stimulus*
1. Binocular double	A + B	C + D
2. " single same field	A	C
3.* " " " "	B	D
4. Monocular same field	B	C
5.* " " "	A	D
6. " different field	C	F
7.* " " "	G	A
8. Binocular single different field	C	H
9.* " " " "	D	F

Ideally each stimulus condition should be compared with every other stimulus condition at least six times. To save time the alternative stimulus conditions, marked with an asterisk, may be omitted altogether and comparisons confined to the following pairs.

Stimulation	*Comparisons*
Binocular v. monocular	Condition 1 with condition 4
Intra retinal v. inter retinal	" 2 " " 4
Same field v. different field (monocular)	" 4 " " 6
" " v. " " (binocular)	" 2 " " 8

Each of these comparisons should be made at least four times, the order of the comparisons being randomised differently for different subjects.

Results.

TABLE IV. Number of judgements of phi as most pronounced

	Binocular	*Monocular*	*Intra retinal*	*Inter retinal*	*Same field*	*Diff. field*
Trial 1						
2						
3						
4						
etc.						

From this table the relative strength of phi under the various conditions may be assessed. These findings should then be compared with the data on time ranges recorded for the same stimulus conditions. Suitable tests of significance may be applied if necessary (e.g. using the class results the degree of agreement between subjects might be tested by finding the coefficient of concordance for the different stimulus condition rank orders provided by the individual subjects).

In addition to these quantitative analyses a summary should be made of the subject's spontaneous comments throughout the experiment. In particular it should be ascertained whether or not the subject perceived an after image of the stimulus lights and/or the apparent movement.

Conclusion. In the light of all the data analysed conclusions should be drawn regarding the possible role of central and retinal processes in the elaboration of apparent movement.

Further theorizing should be postponed until completion of the next experiment.

EXPERIMENT Ic

Whereas the foregoing experiments have been concerned with what for convenience' sake may be described as 'peripheral' parameters of the phi phenomenon the present one carries the investigation one stage further by enquiring into the effect of certain central determinants.

In the main it is concerned with the general hypothesis that in common with other perceptual experience phi phenomenon is subject to influence by such factors as past experience and emotional state. Compatible with this hypothesis it will also be demonstrated that phi cannot be explained in terms of eye movements.

Method and apparatus. The experiment will be conducted in two stages. In the first, phi in a meaningful context will be compared with that in a context devoid of meaning.

In the second stage anxiety ratings will be taken for the subjects concerned.

The apparatus consists of a metal or hardboard disc 24 inches in diameter painted black and driven by a motor whose speed can be controlled by a variac or governor. (If such a motor is not available an ordinary gramophone motor and turntable will suffice.)

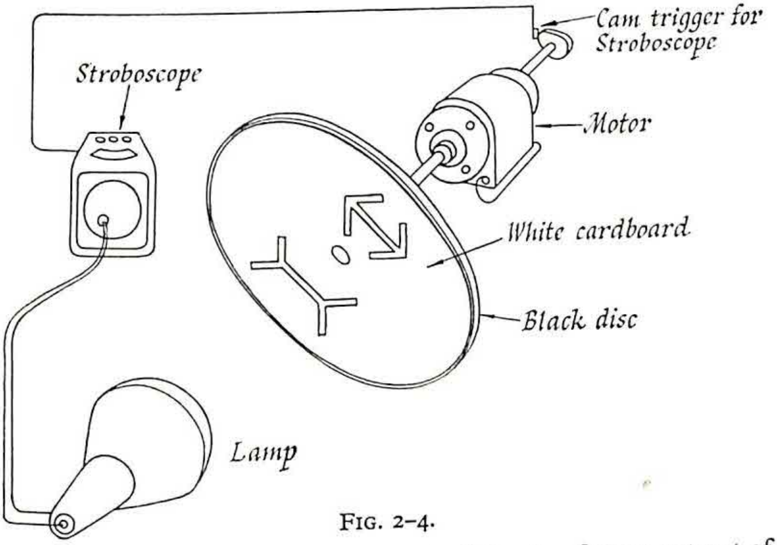


FIG. 2–4.

The stimulus material consists of the following figures cut out of white cardboard and fastened to the disc with clips or Sellotape.

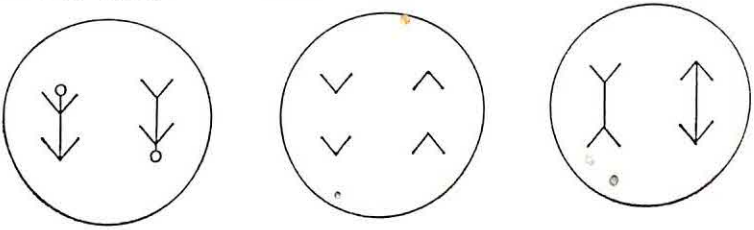


FIG. 2–5.

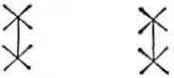
On the main motor shaft that carries the wheel two small cams should be fixed so that each half revolution of the wheel 'makes' a switch contact in series with a stroboscope (a tungsten lamp may be used if necessary, though less satisfactory at high speeds).

Set up in this way and operated in a dark room the apparatus will provide a most compelling double phi phenomenon for every flash of the synchronized light source. (With a 24-inch disc it may be demonstrated to quite a large group of subjects.)

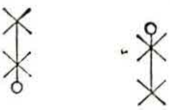
Procedure. The subject is seated in a dark room two yards from and facing the stimulus disc. The experimenter fixes to this disc one of the stimulus figure pairs and sets the disc rotating at a slow speed. The subject is instructed to fixate the knob at the centre of the disc and to report upon his perception of the stimulus figures to right and left of the fixation mark. At slow speeds each will be seen as two figures that alternate with each flash of the lamp. The speed is then gradually increased and the subject asked to indicate when he sees a single moving figure on each side of the fixation point. Thus, the lower phi threshold should be recorded by the experimenter in terms of flash rate or disc speed. (Readings may be taken either from the stroboscope dial or from a tachometer on the disc motor. An even simpler though more laborious procedure is to count the r.p.m. of the disc using a stop-watch.)

During the speed (time interval) range over which phi is experienced the subject should be asked to indicate upon which side of the fixation point apparent movement is most pronounced, i.e. whether in the case of the human figure the arm and leg movements are greatest when the figure is the right way up or when it is upside down.

The speed is then further increased and the subject asked to say when he no longer perceives movement and sees instead a single composite figure either side of the fixation point. In the case of the Müller-Lyer figures the composite figures will both appear like this.



For the human figure they will look like this.



Again the experimenter should record the speed at the transition point.

The speed is then increased still further and then gradually decreased. By this application of the method of limits, the experimenter is able to obtain measures of, respectively, the upper and lower phi thresholds.

This whole procedure is then repeated at least twice for each of the other pairs of stimulus figures. The order of presentation of the three different pairs should be random and differ from subject to subject. During each trial the subject should be asked to say whether the impression of movement is greater or less than that observed on the previous trial with different stimulus figures. To control against place error the relative positions of the two stimulus figures in each pair should be varied from trial to trial.

At the conclusion of the experiment all subjects should be rated for anxiety on the Taylor scale, or some similar test.

Results. The four hypotheses under test – that eye movements cannot account for the phi phenomenon, that phi is sensitive to the same organizational principles which govern the normal static Müller-Lyer illusion, that the meaningfulness of the context in which phi occurs will determine its extent and that the tendency to perceive phi is inversely related to the anxiety level of the subject – may be examined as follows.

Eye movements. For our present purposes it is sufficient here to record the number of times that the subject reports simultaneous movement of the figures on either side of the fixation point. Since the movement on one side of the fixation point is always in the reverse direction from that occurring on the other, simultaneous perception of the two militates against the eye movement hypothesis. Over all subjects the significance of the frequency of reports of simultaneous movement can be found by the sign test.

Müller-Lyer illusion: in the normal Müller-Lyer illusion the line with outspread arrow ends appears longer than that with infolded arrow ends.

If the same organizational principles apply in the context of apparent movement the subject should report a single line which with moving arrow ends appears to stretch and contract like a piece of elastic. How often this occurs and the speed range over which it occurs should be recorded by each subject. The significance of the ratio of subjects reporting this illusion can be arrived at by applying the sign test.

The effect of context. If phi is subject to the same sort of experiential factors as govern most other perceptual phenomena then we would expect it to be more pronounced in a context wherein from past experience we might expect movement to occur than in one without any *a priori* reason for expecting to perceive movement. In the present experiment this general view may be tested in the form of two specific hypotheses. The first is that movement for the limbs of the human figure will be more pronounced when the figure is the right way up than when it is upside down. The second is that the movement of the limbs in isolation will appear less than when they are attached to a head and body.

For an investigation of these hypotheses entries should be made in the following table:

TABLE V. Judgements of greater movement for simultaneously perceived figures

Subject	*Right way up figure*	*Upside down figure*
1		
2		
3		
4		
5		
6		
etc.		
Total		

Since each figure appears equally often on either side of the fixation point the significance of these results may be found by the sign test.

In this table speed ranges constitute the differences between the means of the upper and the lower threshold for each pair of figures. They are of course directly related to the time interval ranges between successive presentations of any given stimulus figure. On the assumption that the greater the time interval range over which it is seen the stronger the illusion, examine the rank order of variables for con-

TABLE VI. Judgements of greater movement based on successive paired comparisons

Subject	*Limbs of human figures*	*Limbs in isolation*
1		
2		
3		
4		
5		
6		
etc.		
Total		

Again test for significance by using the sign test.

TABLE VII. Speed ranges over which phi phenomenon experienced

Subject	*I* *Limbs on human figure*	*II* *Limbs in isolation*	*III* *Müller-Lyer illusion*
1			
2			
3			
4			
5			
6			
etc.			
Total			

sistency between subjects and relative influence of the three conditions. Appropriate tests of significance may be applied where deemed necessary.

The hypothesis that the tendency to perceive phi decreases as a function of anxiety may be tested by correlating the mean of scores in columns 1, 2 and 3 of Table VII for each subject with his anxiety rating on the Taylor Scale. Further tests of a like nature may be carried out on the data in Table VII to find out whether the effect of anxiety on phi is differentially affected by the context in which phi is perceived to occur.

General conclusion. Considered *in toto* the three preceding experiments should enable conclusions to be drawn regarding (*a*) the validity of Korte's laws, (*b*) the role of the peripheral as opposed to central determinants of phi, (*c*) the part played by experiential as opposed to Gestalt factors in the perception of phi, and (*d*) the role of anxiety.

In the light of all these data the student should attempt a discussion of the phi phenomenon in its relation to Gestalt theory, to empiricist theories of perception, to directed state theory, and to Brunswik's theory of probabilistic functionalism. The latter is of course particularly relevant to such findings as the differential in phi strength between the upside down and right way up depictions of the human figure.

EXPERIMENT II. PERCEPTUAL CONSTANCIES

Introduction. There is perhaps no single aspect of perceptual experience which has received more attention by experimental psychologists during the last twenty-five years than that of the so-called perceptual constancies. This remarkable interest in the phenomena concerned may be traced back to an old and from a present point of view surprising assumption, namely that conscious experience should faithfully replicate in certain respects events at the peripheral receptor. Generally the problem of constancy has been that of explaining how it is that the attributes of size, shape, colour and brightness of perceived objects remain apparently unchanged despite variations in the peripheral stimulation whereby their perception is mediated.

A second factor in promoting an interest in the perceptual constancies is that, like the phenomena of apparent movement, they formed much of the subject matter of the old and sterile nativist/empiricist controversy. This controversy centred about the question as to whether the perception of a stable, three-dimensional world, located in depth and filled by objects with relatively constant and unchanging attributes, is learned. Or, being unlearnt, follows from certain principles of organization built into the human nervous system.

Whatever the reasons for its inception, and however meaningless the above controversy, research into the constancies has in fact yielded much information of the greatest relevance to an understanding of perceptual mechanisms, the most important piece of information being that we perceive not merely objects but objects in a spatio-temporal context, wherein the observer (*a*) 'knows' the relationship between the appearance of things apprehended and their stimulus context, (*b*) assumes a constancy for the objects in these relationships and the capacity for lawful change for the stimulus conditions, and (*c*) has a nervous system so constructed as to maintain in conscious experience a constant adjustment of the former for every variation in the latter.

These three considerations define respectively the problems investigated by those interested in perceptual constancies. On the first problem, that of knowing object/stimulus condition relationships, three views have been expressed. The first, that 'knowing' is something built into the nervous system, is the prerogative of the Gestalt school. According to this view, things 'look as they look' (Koffka) not because 'they are what they are' but because the inherent dynamics of the nervous system organize sense data in ways which result phenomenally in stable, change resistant entities. It happens that these phenomenal resultants of extrinsic and intrinsic forces will be of high value from a functional manipulative point of view. The second or empiricist view has been stated in several different ways, the differences here relating largely to the place of the emphasis in designating experiential factors as determinants of constancy. Thus those favouring the 'transactional' approach (Ittelson, Kilpatrick, Cantril) lay particular emphasis upon assumptions derived from a 'weighted average' of previous experiences and present stimulus conditions. Yet others have stressed the importance of knowing the identity and hence 'real' attributes of the object perceived. In one recent experiment (McKennell 1960) for example, it was found that people could estimate the size of an object at a distance just as well by memory as if they actually looked at the object. Over against this, however, Hochberg and Hochberg (1952) have demonstrated an inability on the part of their subjects to locate at appropriately different distances two monocularly presented equi-sized pictures of a man and boy, i.e. objects known *a priori* to be of different sizes. However, while this has been taken to contravert the previous hypothesis it should be borne in mind that memory for the sizes of objects does not bear any necessary relationship to memory for pictures of objects. It could well be that our culturally determined and enthusiastic preoccupation with recording, painting and photographing humans during their growing years has resulted in pictures of children that are, if anything, larger rather than smaller than those of adults.

Yet another way of regarding the effect of experience upon perceptual judgement is that suggested by Vernon (1957). She attaches great importance to 'inferential thinking' based upon schematically organized knowledge. Like Brunswik's (1950) implicit logic of perceptual quasi-influence this type of thinking, according to Vernon, differs from more complex forms of thought, in being 'partially intuitive and even non-veridical', a rapid, rough and ready, often unconscious classification of the immediate percept into some previously established category of experience. The degree to which such processes underlie the 'adjustive' responses observed in constancy experiments will, as Vernon herself points out, depend upon whether the subject is giving so-called 'naïve' object judgements or more analytical sensory judgements. In the latter eventuality, for example, the subject's responses would depend less upon inference than upon, in the case of size constancy, an estimation of the angular segment that the object subtends of the eye. Which type of judgement will be involved will itself depend upon such factors as the experimental instructions, the nature of the situation in which judgements are required (Gilinsky 1955), and the habitual mode of response that the individual because of, say, age or profession tends to adopt.

In this context special mention should be made of recent work by Joynson (1958, 1960). In an attempt to reconcile the apparently divergent views of the early Associationists and the Gestalt school, Joynson has verified the existence of two sorts of perceptual judgement in size constancy experiments and established the conditions under which they are likely to occur. It seems that whether people make (high constancy) 'real' judgements or (low constancy) 'as things just look' judgements depends very largely upon the angular separation between the objects compared. Moreover the everyday 'real' judgement response in which people judge size, having taken distance into account, thereby achieving a high degree of constancy, is not, as the Gestaltists maintained, an immediate perceptual experience, nor as the Associationists thought wholely the result of past experience, but rather an *activity* combining both past and present experience.

On the other hand the less usual 'not real' or 'as things just *look*' judgement which is made in terms of relative retinal image sizes seems, from Joynson's subjects, to be a more passive response involving 'sheer eyesight' or 'mere appearance'. As might be expected this sort of response in which distance has to be ignored is easiest with simultaneous comparisons at low angles of separation between the objects concerned.

Finally in this brief exposition of different approaches there is that favoured by Gibson. Gibson's most important contribution to the psychology of the constancies is that he draws attention to those retinal correlates of the total stimulus field by which the essential cues of distance, orientation and illumination are mediated. According to Gibson such cues as texture, gradient of intervening ground, surface structure, abrupt transitions between one surface and another, and light-dark relationships in the mosaic visual field, provide the data upon which the visual world of constant objects is based. While appearing very different, Gibson's views and those expressed by Vernon are not in fact mutually exclusive. To generalize, the former would seem to explain the similarities, the latter the differences, between the perceptual experiences of different individuals.

Within the context of these differing theoretical viewpoints, the role of both stimulus and observer variables has been studied extensively. (In recent years particular attention has been given to a reanalysis of the invariance hypothesis. This hypothesis is based upon the known relationship between physical size, distance and visual angle. It argues mistakenly that what is true physically must also be true psychologically, i.e. that size constancy results from the (invariant) relationship between apparent size and apparent distance. For a detailed discussion of this problem the reader is referred to *Visual Space Perception* by W. H. Ittelson, 1960.)

Since shortage of space forbids a review of these many and varied studies, the student is referred to the following abbreviated directory. Though not exhaustive, it includes a selection by the writer of what he believes to be the key experiments and, in addition, a number of reviews through which other studies may be traced.

PERCEPTUAL CONSTANCIES

Size

Thouless 1931 Woodburne 1934 Martin & Pickford 1938 Holway & Boring 1941 Gilinsky 1951 Hochberg & Hochberg 1952–3 Ittelson 1952–3 Ittelson & Kilpatrick 1952 Gruber 1954 Gogel, Hartman & Harker 1957 Rump 1960 Oldfield 1960	Size/distance relationship
Taylor & Boring 1942 Boring 1943 Dadourian 1946 Leibowitz & Hartman 1959	Moon illusion
Joynson 1949 Joynson 1958 Joynson 1960 Hastorf 1952 Hochberg & Hochberg 1952–3 Bolles & Bailey 1956 Vernon 1957 Jenkin & Hyman 1959 McKennell 1960	Central factors, attitude, suggestion, familiarity, and cognitive inference
Weber & Bicknall 1935 Beyrl 1936 Sheehan 1938 Leibowitz, Bussey & McGuire 1957	Stimulus object differences
Brunswik 1944 Brunswik 1956 Dukes 1951	Situational factors
Brown 1954 Hermans 1954 Roelfs & Zeeman 1957 Heinemann, Tulving & Nachmias 1959	Miscellaneous 'peripheral' factors; convergence, pupil size, monocular versus binocular viewing
Gregory, Wallace & Campbell 1959 Whiteside & Campbell 1959 Oldfield 1960	Changes in viewing position, angular and radial acceleration, formulae for size constancy
Thouless 1932 Holaday 1933 Henneman 1935 Locke 1938 Sheehan 1938 McKennell 1960	Individual differences
Köhler 1915 Beyrl 1926 Burzlaff 1931 Thouless 1933 Beveridge 1935 Locke 1937 Brunswik & Cruickshank 1937 Piaget & Lambercier 1943. Piaget 1945–6 Dukes 1951	Comparative studies – child and animal

Shape

Thouless 1931
Ames 1946
Eissler 1933
Sheehan 1938
Stavrianos 1945
Klimpfinger 1933

Buytendijk, Van Ratingen 1951

Makino 1950
Gottheil, Edward & Bitterman 1951

Colour and Brightness

Phenomenal regression	Hering 1905	Structural relationships in stimulus field
Rotating trapezoid	Jaensch & Müller 1920	
	Gelb 1930	
Regression/orientation relationship	Kardos 1934	
	Wallach 1948	
	Cramer 1923	Figural properties of stimulus field
Constancy of an ellipse	Granit 1926	
	Katona 1929	
	Macleod 1932	
	Henneman 1935	
Methodology	Helson 1938	
	Macleod 1940	
	Hsia 1934	Effect of brightness gradient
	Macleod 1947	
	Wallach 1948	
	Helson 1938	Contrast and constancy
	Helson 1943	
	Wallach & Galloway 1946	
	Stewart 1959	
	Katz & Révész 1921	Comparative studies – child and animal
	Burkamp 1923	
	Gotz 1926	
	Brunswik 1929	
	Burzlaff 1931	
	Hering 1905	Cognitive factors
	Adams 1923	
	Katz 1935	
	Duncker 1939	
	Bruner, Postman & Rodrigues 1951	

General, and Reviews of Constancy Experiments

Katz 1930
Koffka 1935
Vernon 1937
Boring 1942
Vernon 1952
Stevens 1951
Osgood 1953
Gibson 1955
Woodworth & Schlosberg 1955
Taylor & Papert 1956
Brunswik 1956
Ittelson 1960

Miscellaneous

Uexküll & Kriszat 1934	Perceptive experience without 'thing constancy'
Mohrmann 1939	Loudness constancy
Fischel 1926	Weight constancy
Izzet 1934	Size weight illusion

The present experiment. While very many experiments have been done to establish the essential variables in the occurrence of constancy phenomena, they have for the most part concerned themselves each with only one type of perceptual constancy. Though admirable from the point of view of simplification, this procedure has necessarily involved situations which depart considerably from those encountered in normal day-to-day perceptual experience. Constancy for size, shape, colour and brightness does not occur as a number of separate events but as a single unifying experience. Despite the fact that we experience a world of objects whose retinal images vary continuously along dimensions of size, shape and brightness, so in our awareness of these objects do they tend to remain unchanging in all these three respects. For this reason it has been thought advisable to introduce an experimental approach to the subject of constancies by outlining an investigation which it is felt approximates more closely to the real life situation. The crux of this experiment will be that the subject is required not merely to match for size or shape a variable and a standard stimulus patch, but to select from a given set of possible objects varying continuously within limits along the essential dimensions that which he thinks most nearly identical to a given object having certain attributes of size, shape and brightness. Though it is not easy to compare, on a quantifiable basis, degrees of constancy along different dimensions, such an experiment should provide a demonstration of what is involved in the three aspects of constancy and also an indication of the relative importance and accuracy of the three sorts of constancy judgement which the subject will be required to give.

Hypothesis. (1) In adjusting a variable object to match a given standard object, under different conditions of distance, orientation and illumination, S will show a tendency to select a variable object having the same size, shape and brightness as the standard. (2) Constancy will be greatest for size, less so for shape and least of all for brightness. (3) In respect of shape and size, constancy will be greater for a previously experienced meaningful standard object than for one devoid of particular meaning. (4) Constancy in respect of all three dimensions will be greater when S receives cues as to distance, orientation and illumination than when these are denied.

Apparatus and method. The major problem in research of this kind is that of devising variable and standard objects which can be changed continuously along all three of the continua with which we are concerned.

To this end, the writer has found the following apparatus of considerable value:

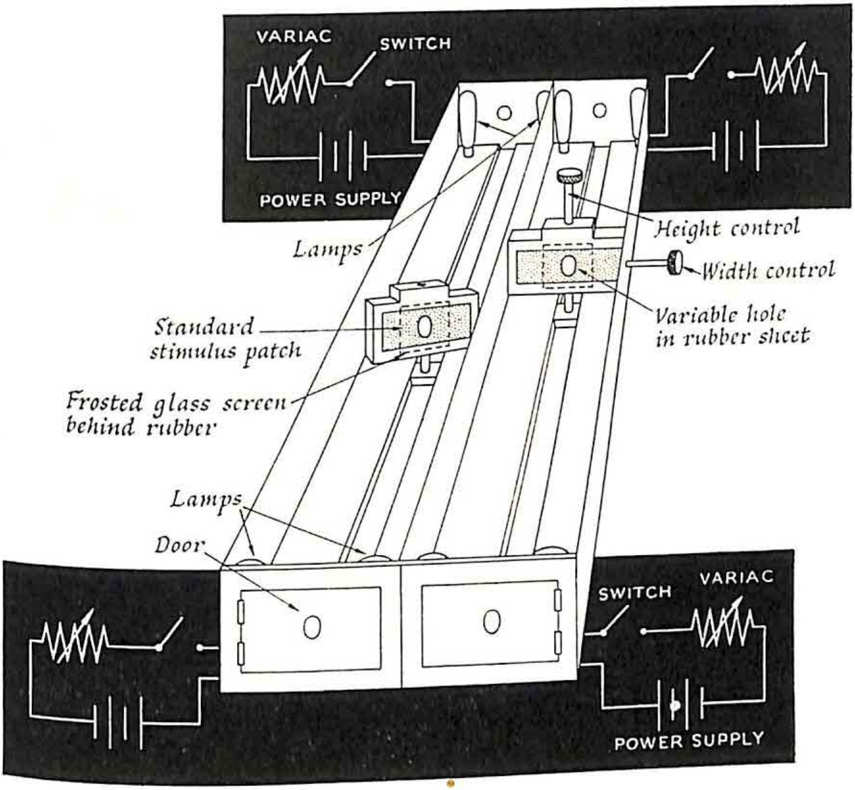


FIG. 2–6.

It consists of two rectangular boxes 36 × 5 × 5 inches, each with doors at either end. Each door contains a circular aperture which may be plugged with a cork. The boxes or alleys contain a longitudinal slide, carrying the two test objects such that these may be positioned independently at any given distance down the box. On the standard side the test object consists of a square of opaque rubber containing a central hole the size of a penny. The rubber is kept permanently stretched within a wooden frame that may be rotated on its vertical axis as well as shifted back and forth in the alley. Behind the rubber is a sheet of frosted glass and behind this two lamps fitted in the far corners of the box. The arrangements on the 'variable' side are much the same except that the sheet of rubber containing the test hole may be stretched differentially in two directions. This vertical and horizontal stretching which is achieved by two control knobs on the outside of the box enables the size of the central aperture to be varied continuously for both size and shape. As with the standard, the variable test patch (or hole) is backed by frosted glass illuminated from the rear by a pair of lamps.

In front of both the test objects are placed two further pairs of lamps. The brightness of all four pairs of lamps may be independently controlled by four variacs.

To measure constancy for size, shape and brightness the standard test object is held constant at a certain distance down the alley and turned out of the fronto-parallel plane by a given number of degrees. The door behind the test patch is closed and the lights behind it adjusted to give the patch a certain level of luminance. The door at the other end of the alley is opened and the lights in front of the test patch set to give a certain level of illumination. In the other alley the variable test patch is set in the fronto-parallel plane but at a different distance down the alley from the standard. As with the standard, the rear door is closed and the front door opened. The general level of illumination is set at a value different from that falling on the standard. Under these conditions the subject's task is to adjust the size, shape and brightness of the variable patch until it is phenomenally equal to the standard. The fact that the apparatus is symmetrical, having doors at both ends, means that the whole thing can be turned round, thus changing the spatial relationship of the standard and variable stimulus patches. Finally the small circular apertures in each of the doors enable constancy measures to be taken under reduced cue conditions.

Procedure. The subject is seated with his head on a chin rest, and his eyes on a level with, and some convenient distance from, the front doors of the box. The standard stimulus patch is set at 45 degrees to the fronto-parallel plane and 18 inches from the front of the box. The illumination of the standard is set at x* log foot lamberts (as previously calibrated on the variac and its luminance at y log foot lamberts).

The variable is set in the fronto-parallel plane 30 inches from the front of the box. Its internal dimensions, as determined by the two screwed controls, are set at the extremes of smallness in height and width, i.e. it is seen as a disc very much smaller than the standard. Its illumination is set at 2 log foot lamberts and its luminance minimally low. The subject is given the following instructions. 'I want you to make the figure on the right look exactly like that on the left. As you see, it can be altered in size, shape and brightness, by operating these three controls until the two figures look identical.'

While the S is making the necessary adjustment a note should be made of the order in which these are carried out and the total time involved. As an example the protocol should be of this order:

1. Increased height of variable
2. Increased width
3. Decreased height
4. Increased height
5. Increased brightness
6. Increased height
7. Decreased brightness
8. Reported finished. Total time 12 minutes.

The alleys are then closed and the three control readings recorded. For the next trial the variable test patch is preset to be maximally bright and large, the subject making his adjustments as before. After six such ascending and descending trials the apparatus is turned round and the same procedure carried out with the variable on the right and standard on the left. (Half the subjects should have the order of positioning reversed.) This constitutes the basic procedure under open cue conditions. If time permits the entire procedure should be repeated with the standard set at different distances, orientations and brightnesses.

For reduced cue conditions the procedure is the same except that now the leading doors are closed and the circular apertures within them opened. Half the subjects tested should have the open cue conditions first, the other half the reduced cue conditions.

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* Suitable values should be empirically determined by the instructor beforehand.

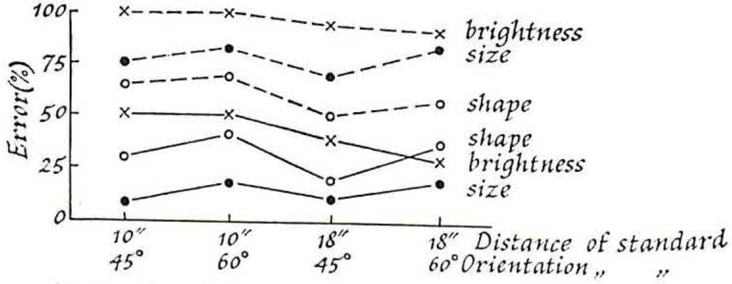
To investigate the effect upon size and shape constancy of knowledge of the real size of the standard, a further six ascending and descending trials, with variable alternated from right to left, should be carried out with a coin substituted for the standard test patch. It may be held in place by a smear of photopaste on the frosted glass backing to the stimulus patch. For these trials illumination is held constant for both standard and variable and the latter set at minimal luminance.

Introspective data should be collected at the conclusion of each block of trials.

Results. With a view to examining our four hypotheses the data should be gathered in Table VIII.

From these data collected over all subjects the experimenter should compute the percentage errors (departures from constancy) for the three dimensions of the variable under, respectively, the four conditions of distance and orientation of the standard.

Curves as exemplified below should be plotted comparing these data with those obtained under reduced cue conditions.



(Broken lines indicate reduced cues)

FIG. 2–7.

The data for size and shape constancy under both open and reduced cue conditions should then be compared with those obtained when a meaningful object as standard, namely a penny, is substituted for the simple test patch.

When sufficient subjects have been tested, the effect of reducing cues on the one hand and of using a meaningful object on the other should be tested for significance.

TABLE VIII. Mean settings of variables for various standards under open and reduced cues

	Open cues							
	S	V	S	V	S	V	S	V
Distance	18″	30″	10″	30″	18″	30″	10″	30″
Orientation	45°	Fp	45°	Fp	60°	Fp	60°	Fp
*Illumination	3	2	3	2	3	2	3	2
Height	1″		1″		1″		1″	
Projected width	·5″		·5″		·3″		·3″	
*Luminance	5		5		5		5	
Width/height ratio	·5		·5		·3		·3	
	Reduced cues							
Distance	18″	30″	10″	30″	18″	30″	10″	30″
Orientation	45°	Fp	45°	Fp	60°	Fp	60°	Fp
*Illumination	3	2	3	2	3	2	3	2
Height	1″		1″		1″		1″	
Projected width	·5″		·5″		·3″		·3″	
*Luminance	5		5		5		5	
Width/height ratio	·5		·5		·3		·3	
	Meaningful object							
Distance	18″	30″	10″	30″	18″	30″	10″	30″
Orientation	45°	Fp	45°	Fp	60°	Fp	60°	Fp
*Illumination	3	2	3	2	3	2	3	2
Height	1″		1″		1″		1″	
Projected width	·5″		·5″		·3″		·3″	
Width/height ratio	·5		·5		·3		·3	

S = standard; V = variable; Fp = fronto-parallel plane.
* Any convenient unit.

Conclusions. In the light of the quantitative data, the procedure followed by the subject during the experiment, and his subsequent report of introspections, the experimenter should be in a position to consider the four hypotheses upon which the research was based. In particular he should attempt answering the following questions: to what extent are the different aspects of object constancy parts of a single perceptual experience? Are 'adjustments' for 'shape', 'size' and 'brightness' executed separately and in order, the subject responding first to this set of cues and then to that, or do they all occur together, the subject just choosing that object most like in all respects the real standard. Again to what extent are the various constancies a function of conscious deliberation and to what extent of immediate unconscious inferential processes? And are there individual differences in these respects?

Inasmuch as this was a first experiment, investigating at one go, if in a rather cursory way, all three aspects of perceptual constancy, the experimenter should give some thought to (*a*) interactions between the different processes involved (e.g. does the cue of reduced illumination which facilitates brightness constancy militate against size constancy, because misleading as to the distance of the variable object?) and (*b*) ways whereby these interactions may be investigated in greater detail.

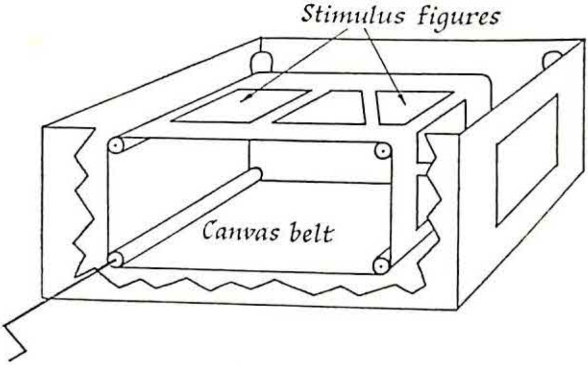


FIG. 2–8.

Finally, the experimenter should attempt a review of his data in the context of previous research, giving amongst other things a critical analysis of the pros and cons of adopting the multi-dimensional approach advocated here.

Note. Apropos of the experiment just described it may be thought, with some justification, that since size, shape and brightness are handled independently of each other the situation still does no more than approximate to that encountered in normal perceptual experience. With this in mind the following alternative method might be preferred. In this the continuously variable rubber disc is replaced by a hand drive canvas belt upon which are fixed in a random order a selection of figures that vary in size, shape and whiteness over a range whose extremes are equidistant either side of the standard.

When one is using this apparatus, the procedure involves getting the subject to select from the large range of possible figures that which 'looks most like' the standard. In using this method a compromise must be achieved between (1) so many figures that selection is long and arduous and (2) so few that only the very coarsest constancy measurements can be obtained. Over and against its obvious limitations the method has the considerable advantage of forcing the subject to handle all three dimensions simultaneously, thus closely imitating the situation that obtains in normal every-day perceptual experience.

Procedure for a group experiment. While investigations of constancy phenomena do not in general lend themselves to group experiments the following procedure may be adopted where there are insufficient pieces of apparatus for individual experiments. The alleys are separated from each other, the one being placed some distance behind or in front of the other. Differences in illumination are obtained by placing a partition between the alleys, thereby screening one of them from some main source of illumination. With the leading doors open both the standard and variable test patches are brought to the front of their respective alleys.

With the standard at a given size, orientation and brightness the experimenter then varies in a predetermined manner the three dimensions of the variable. Under these conditions the subjects are merely required to record for each variation made by the experimenter whether the variable looks larger than, smaller than, or equal to the standard; more, less or equally elliptical; and with equal, more or less luminance.

Provided the entries on the subjects' score cards can be accurately matched up with the actual values of the variables fed in by the experimenter then the results may be handled in the same way as for the individual experiments.

EXPERIMENT III. WORD RECOGNITION

Introduction. As a part of language behaviour the recognition of verbal material is probably man's single most important perceptual experience.

Inasmuch as it involves learning and retention, the receptive processes of perception, and a variety of overt responses, both central and autonomic in origin, recognition has been studied in a variety of contexts. In many of them the dependent variable is the accuracy with which a subject can verbally report upon the nature of verbal material under various conditions prescribed by the experimenter. The stimulus variables which have been found to affect the recognitive process are: the context in which the word appears (Bruce 1955), the familiarity of the word (Solomon and Howes 1951), the selective information content of the word (King-Ellison), and, of course, the duration or intensity of the verbal stimulus. By far the greatest body of research in this area has, however, been devoted to yet another parameter of the verbal stimulus, its affective aspect. Since the original experiment by Postman, Bruner and McGinnies (1948) a number of studies has suggested that the parameter of emotional value exerts a considerable influence upon the recognition threshold for a particular word. McGinnies (1949), for example, showed that people presented with words in a tachistoscope require a significantly longer exposure time to report such words as 'bitch' and 'penis' than they did other more neutral words. He showed, moreover, that even prior to recognition such tabu words evoked larger galvanic skin responses than their control counterparts. Findings such as these gave rise to the concepts of perceptual defence and perceptual vigilance, concepts with the theoretical implication that, prior to conscious recognition, words are unconsciously recognized and filtered for ease or difficulty of recognition, McGinnies' view being in fact that so called anxiety words constitute conditioned stimuli for an avoidance response, and that it is the autonomic response which precedes conscious awareness that activates the defensive mechanism of raised threshold. This theory of perceptual filtering has, however, met with considerable opposition on three counts.* In the first place it has been deemed anthropomorphic nonsense (Howie 1952) to imagine that an individual can recognize a word at one level so as not to recognize it at another. In the second place, it has been maintained (Solomon and Howie 1951) that threshold differences attributed to differences in the emotionality of the words presented are more parsimoniously explained in terms of differences in familiarity as between neutral and tabu words.

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* For critical reviews of experiments on perceptual defence, etc., see recent papers by Eriksen (1960) and W. P. Brown (1961), *Brit. J. Psychol. Monog.*, **35**.

Finally, it has been claimed with some justification (Whittaker and Gilchrist 1952, and Postman, Bronson and Gropper 1953) that what looks like perceptual defence for a 'rude' word is often in fact voluntary suppression of response occasioned by the embarrassment of having to utter the word in question.

Notwithstanding these criticisms, there remains experimental evidence (Blum 1956; Dixon 1960; McGinnies and Sherman 1952) for threshold changes due to the affective nature of the stimulus which defy easy explanation by either the familiarity or the suppression hypothesis. Before introducing the techniques involved in these investigations it is however worth giving a thought to the *sort of mechanisms* involved in threshold regulation. They must have at the very least the following attributes: (1) the ability to achieve at least partial discrimination of words at a level below that required for full conscious recognition, (2) as a result of such discriminations, the power to determine in a differential fashion the awareness threshold for different classes of words. In an attempt to substantiate the reality of such a mechanism Lazarus and McCleary (1951) carried out an experiment which suggested that the conditioned G.S.R. could occur to nonsense syllables exposed so briefly as to preclude their correct verbal report. Though criticized by Howes (1954) on the grounds that the results could be attributed to partial recognition of the syllables, the experiment was important in making explicit the notion of a hierarchy of thresholds without which any theory of perceptual defence is difficult to sustain.

A second experiment with important implications for any theory of recognition threshold regulation is one reported by Ulric Neisser (1954). This study was primarily concerned with distinguishing the perceptual and overt-response aspects of the recognition process. Neisser was able to show that while recent familiarity with selected words facilitated their subsequent recognition, it in no way assisted recognition of their homonyms. Since verbal response to a word is identical with that to its homonym the implication of this finding is that the familiarity effects reported by Solomon and Howes apply directly to the perceptual experience which may be considered in isolation from its verbal reporting.

While the precise role of central determinants, particularly those of an emotive or personality-based kind, remains extremely controversial the value of the aforementioned studies lies, to quote Osgood (1953), 'not so much in the specific facts they have uncovered but rather in the extraordinarily fruitful research ideas they have propagated: the idea that personality dynamics influence even the simplest perceptual activities and can be detected in the controlled conditions of classic experiments'.

Before embarking upon a description of an experiment to illustrate a number of the points referred to above, there are however one or two considerations worth bearing in mind. In the first place, the phenomenon of threshold differentials traceable to differences in emotional loading of the stimulus items does not necessarily imply either the notion of an autonomic threshold lower than that for conscious awareness nor the notion of a super-sensitive, super-discriminating unconscious which having detected the stimulus word 'decides' whether or not this should be 'permitted' entry into consciousness. Inasmuch as it is perfectly possible to construct a machine which at one level can filter messages for transmission to a higher level on the basis of the nature of the messages, it seems equally feasible that the hierarchical structure of the nervous system should be capable of this function. In the second place, inasmuch as the evidence suggests that threshold regulation is a purely statistical effect, the grossest discriminations of words, *likely to be of an emotive kind because they are sufficiently similar to certain emotive words*, should be enough to account for the results recorded to date in perceptual defence experiments. This latter view, advanced by Lazarus (1956) and Osgood (1957) respectively, has been criticized by Eriksen on the grounds that since certain emotional words differ by only one letter from certain neutral words, only the finest of discriminations could distinguish between the two. To this there are two answers. First no one has yet suggested that people can in fact discriminate between such words as Eriksen refers to. Secondly there is evidence (Dixon and Boulos 1960)* in support of the gross discrimination hypothesis, to the effect that faced with a disfigured or impoverished and hence only partially recognizable word, which in fact resembles an emotional word, people will react to it *as if it were* an emotional word.

* Unpublished research.

A second consideration to bear in mind when experimenting in this area is that the phenomena of perceptual defence and vigilance are no longer completely inexplicable, let alone 'impossible', from a physiological point of view. Recent work, as reviewed by Samuels (1959) on the reticular activating mechanisms of the brain stem, suggests that there are feedback circuits within the central nervous system whereby it is possible for the cortex to control not only the transmission of impulses in sensory relays 'further downstream' but also its own level of arousal. By such mechanisms it would be theoretically possible for 'messages' to reach the cortex without awareness by the individual concerned and as a result of such reception to either block further inputs, or to set its own threshold for their conscious recognition.

Our third consideration refers to a question of methodology for research in this area. The point here is that much earlier work was to some extent vitiated by use of the tachistoscope. Inasmuch as the subject is aware of the stimulus long before he can signify correct recognition by verbal report it is difficult to ascertain the extent to which 'pre-recognition' responses are in fact below the awareness threshold. As maintained in an earlier research (Dixon 1958) the writer inclines to the view that this difficulty, inherent in the tachistoscopic method, can be overcome by using intensity rather than duration of the stimulus as the independent variable in recognition studies. By combining this method with that of simultaneous double stimulation wherein a verbal stimulus is presented below threshold to one eye while a neutral test patch is presented to the other eye, at an intensity which can be controlled by the subject, it is possible to investigate the hypothesis of perceptual defence under conditions less equivocal than those used hitherto.

The final point of relevance to our demonstration experiment concerns the phenomena of pre-recognition responses. During the course of experiments involving the presentation of verbal stimuli on or below the recognition threshold several workers (McGinnies 1949; Dixon 1958; Goldstein 1960) have noticed a tendency for a semantic rather than structural link between pre-recognition guesses and the actual stimulus item. The fact that many of these 'associations' have a decidedly autistic flavour raises some interesting questions for future research, and for perceptual theory in general. For one thing they suggest perhaps that the genesis of a perception parallels that of thinking in that the final product owes as much to suppression of distant associates as to the formulation of what is consciously selected and intended. Fisher's (1954) finding that the non-reported part of tachistoscopically presented pictures reappeared as a part of subsequent dream symbolism would seem well in line with this speculation.

The foregoing outline of work and methodology applied to the study of recognitive processes embraces much that features in the following experiment. As with the two previous experiments described in this chapter, this investigation for the laboratory class is intended primarily as a demonstration of what is involved in verbal recognition and, secondly, for the collection of data on the essential parameters.

The experiment. Hypotheses. 1. The recognition threshold for verbal material will (*a*) vary inversely with the familiarity of the stimulus items; (*b*) vary directly with the selective information content of the stimulus items; (*c*) be significantly higher or lower for items that are emotionally loaded.

2. The difference threshold for a neutral test patch presented to one eye will vary as a function of the emotionality of words presented below threshold to the other eye.

3. Responses given before the correct one will be meaningfully related to the words presented.

Apparatus. The apparatus is easily adapted from that used in the preceding experiment. It consists of two alleys containing movable lamps which can be brought forward by the subject. Between the lamps and the end of the alleys are two slide holders, and in front of these two frosted glass screens. These latter are divided into four quadrants in any one of which the test patch can be presented.

General method. For the hypotheses I and III the method involves recording S's responses regarding the nature of stimulus material in one of the slide holders while the appropriate lamp is brought gradually nearer the front of the box. The advantage of using this method of intensity control is that it avoids the complication of filters while at the same time, unlike variac controls, maintaining relatively unchanged the spectral composition of the light incident at the glass screens. Gross intensity changes may likewise be obtained by substituting lamps of higher or lower wattage.

For hypothesis II the method involves measurement of the least light intensity required by the subject to see a test patch exposed to one eye while different sorts of stimulus material are presented above and below threshold to the other eye.

For all three hypotheses, the stimulus material should be photographed on 35-mm film or typed on to translucent paper.

It is, of course, apropos of the stimulus material to be used that any investigation involving meaning or content as an independent variable runs into its greatest difficulty, that of adequate quantification of the crucial attributes of meaning. In the present experiment the crucial attributes are of course familiarity and emotionality. Faced with this problem there are in fact three ways of handling the selection of stimulus material, each being at best a compromise. First, we could select actual words quantified for familiarity and emotionality on the basis of (1) a statistical analysis of the language for the former, and (2) a set of general beliefs regarding the latter. A snag here is of course that those very words which one feels intuitively are most certain to be universally emotional tend not to appear in word frequency counts. A second method would be to ascertain for each subject by means of say word association tests those particular words which for him are emotionally toned, words which, sufficiently common to appear in frequency counts, may then be matched with neutral words of the same length and frequency. Finally, a quite different approach is that of using nonsense syllables or paralogs, some of which have been previously associated with some unpleasant emotional experience such as electric shock. This using of hitherto unfamiliar items as conditioned stimuli for an emotional response enables perfect control of the familiarity and emotionality variables but being a somewhat artificial situation provides at the best only the most indirect evidence for the nature of normal recognitive processes.

In the following experiment we propose adopting a compromise between the first two methods. The experimenter selects words that he regards as emotionally toned, with neutral control words of the same frequency, and subsequently checks with the subject regarding the actual familiarity and emotionality of the words that were used.

For the subsidiary hypothesis regarding selective information content we are forced to use the third method or paralogs since it is only by this means that selective information as defined by Shannon (1949) can be accurately quantified.

Procedure. Part I. Using the Thorndike-Lorge word count the experimenter should prepare a list of ten five-lettered emotionally toned words varying in frequency from very rare to common. For each of these he should select a neutral counterpart of the same length and frequency. A typical pair might be:

Emotional category	*Neutral category*
'Cancer'	'Divers'

Each of these words should be written, or better still typed, on a rectangle of translucent paper suitable for insertion into the slide holders. The total set of twenty stimulus items should be arranged in a random order and entered in the same order upon a data sheet on which the experimenter will record pre-recognition 'guesses' and recognition threshold intensities. A similar list of randomly selected positions for the test patch should also be prepared. Using any five-lettered word not on the above list the experimenter should then ascertain, roughly, the subject's recognition threshold, so adjusting the lamp wattage that the threshold point occurs when the lamp is approximately half-way along the alley. This same procedure should be carried out for the test patch in the other alley. This patch consists of a pin-hole placed off centre in an otherwise opaque card inserted in the other slide holder. By inserting layers of translucent paper against the patch in the same holder the experimenter should so arrange the intensity of the test patch that the subject can just see it when the lamp is half-way along the alley. In which of the four randomly selected quadrants the patch appears on any one trial is of course determined by the orientation of the slide in its holder. In both alleys calibration of the lamp moving mechanism should be carried out by photometer readings at the point of maximum illumination followed by application of the inverse square law which states that illumination of a surface is an inverse function of its distance, squared, from the light source.

Both lamps are then set at their furthest distance from the two screens and the subject given the following instructions: 'Every time I say "now" I want you to guess regarding the word which I shall present, at a very low intensity, to your right eye. If you find it difficult to make a guess just say the first word that comes to mind having looked down the alley. At the same time I want you to tell me when you can first see the spot of light which I shall present to your other eye.' The first slide is then put in the holder. The experimenter says 'Now' and records the time that elapses between this signal and the subject's response. He then increases the intensity of the test patch and records the point at which the subject reports correctly upon its location in one of the four quadrants marked on the screen. The light in the alley containing the verbal stimulus is then advanced one unit on the distance scale (the ideal distance for each step to be found by a pilot experiment) and the procedure repeated as before, the test patch being adjusted on each trial to threshold intensity from values above and below this point. The entire cycle of events is repeated until such time as S recognizes the word, taking as a criterion the correct reporting of it on three successive responses. The whole procedure is repeated for all the words.

The next stage of this experiment is devoted to ascertaining the S's estimation of the relative familiarity and emotionality for him of the stimulus items. A quick procedure here would be to require him to rank the words in order for (*a*) 'the frequency with which he thinks he encounters them in reading' and (*b*) 'the degree to which he finds them productive of unpleasant emotion'. A better though more lengthy procedure would be to use the method of paired comparisons wherein each word is compared with every other according to the criteria contained in the instructions.*

In the final stage of this part of the investigation the experimenter should attempt to find out whether or not the pre-recognition hypotheses are causally related, either in structure or from a semantic point of view, to the stimulus items whose conscious apprehension they preceded. Since the responses were in fact verbalized rather than written, the best matching procedure would be to read the subject's responses back to him in random order while he calls out the number of that stimulus item (from a prepared randomized list of the latter) which he associates most strongly with the word in question. The idea of assigning numbers to the stimulus items is merely to control against the possibility of the subject avoiding report of stimulus words which he might find distasteful to utter.

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* It is important to stress to the subject that his judgements should be in no way influenced by his ease or difficulty in recognizing the words.

Subsequently the experimenter should go through the list of matches in order to ascertain where possible the reason for their occurrence.

Part II. For the second part of this experiment designed to investigate the effect of selective information content upon recognition threshold, it is suggested that the experimenter follows the procedure adopted by King-Ellison (1953) wherein the subject's recognition threshold is found for a series of five letter paralogs* whose *a priori* familiarity has been fixed in a pre-test session. According to this method the subject is first asked to spell and pronounce a hundred paralogs presented each on a separate card. Within this pack of made-up words, which the subject is led to believe are foreign, two paralogs occur twenty-five times, two occur ten times, two five times, two twice, and two once each, thus making a total of 86 words. Fourteen dummy words are added to make up the total to a hundred. Within a group of subjects each of the experimental words is used at each frequency level in a counter-balanced design, the order of presentation in each pack being random.

Following the pre-test session the subject reads unrelated material for twenty minutes after which his recognition threshold is found for four practice paralogs and then the ten experimental items.

For determining recognition thresholds the experimenter may use either a tachistoscope, proceeding in 30 ms. steps on successive exposures from an initial exposure duration of 30 ms. Better still, he may use the apparatus already described. When using the latter apparatus the same procedure should be employed as in Part I of the experiment except that now only a single alley is used, the subject no longer being required to maintain the point source of light (in the other alley) at threshold level. The experimenter should record pre-recognition 'guesses', the threshold duration (or intensity) for each paralog and the subject's subsequent introspections. As to the latter he should be particularly concerned to find out whether the subject was aware of the true purpose of the experiment, whether in fact he realized that some of the paralogs were more familiar than others and because of this more easily recognized.

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* Examples are given in Dunlap's list reprinted in Woodworth (1938).

Results.

TABLE IX. Thresholds for point source and recognition of verbal stimulus

Stimulus words		*Threshold intensity for point source at increasing intensities of verbal stimulus intensity steps*										*Threshold intensity for recognition of verbal stimulus*
		1	2	3	4	5	6	7	8	9	10	
E	1											
	2											
	3											
	4											
	5											
	6											
	7											
	8											
	9											
	10											
N	1											
	2											
	3											
	etc.											

Having collected the data in the above table the experimenter should investigate the various hypotheses in the manner outlined below.

1. Effect of familiarity on recognition threshold: (*a*) curves should be plotted showing recognition thresholds as a function of frequency ratings on the Thorndike count, and as a function of the subject's ratings; (*b*) the significance of such relationships as are observed should be tested by computing rank correlation coefficients.

2. Effect of 'emotionality' of stimulus items upon the awareness and recognition thresholds: As in the case of 'familiarity', curves should be plotted showing the relationship between emotionality and: (*a*) recognition thresholds; (*b*) mean of awareness (point source) thresholds; (*c*) response times.

In addition, curves should be plotted to show changes in the awareness threshold as a function of intensity changes below the recognition threshold. One such curve would be for the mean of all the emotional words at each intensity level, the other for the mean at the same intensity levels, of all the neutral words.

Over a group of subjects the 't' test for related means may be used to ascertain the significance of differences in both awareness and recognition thresholds as between the two sorts of stimulus material.

If material below the awareness threshold, as a function of its emotional import, affects the threshold, we should expect differences in the latter, as between emotional and neutral words, to increase with increments of stimulus intensity. This again may be tested by application of a suitable rank correlation test.

3. Pre-recognition 'guesses': the hypothesis that pre-recognition 'guesses' are in fact determined by the below threshold stimulus item may be tested by applying Stevens' test (1939) to the matchings made by the subject at the conclusion of the first part of the experiment. In addition to this statistical test, the experimenter should attempt a qualitative analysis of the subject's responses to determine, if possible, whether the link between stimulus and response is structural or semantic, and whether or not there is any difference between 'emotional' and 'neutral' stimulus words in their effect upon pre-recognition 'guesses'.

4. Effect of selective information content of stimulus on recognition threshold: Part II of the experiment was designed to investigate the effect of 'familiarity' in the special context of information theory.* There were two reasons for this. In the first place, it enables precise quantification of the essential factor in familiarity, the prior probability of occurrence of a particular symbol. In the second place the great generality of communication theory with its concepts applicable to all situations involving information transmission makes it highly relevant to all those areas of psychology wherein it is useful to regard

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* By way of an introduction to this subject it is recommended that the student read Weaver's contribution to *The Mathematical Theory of Communication* by Shannon and Weaver (1949), MacKay's paper 'In Search of Basic Symbols' in the Josiah Macy *Symposium on Cybernetics* (1952), and *Applications of Information Theory to Psychology* by Fred Attneave (1959).

the organism as a communication channel. In the present context 'if', to quote King-Ellison,

> the perception situation is considered as a communication channel through which symbols are being transmitted to the subject, as a destination, the known prior frequency of these symbols may be tested as an index of the amount of 'information' they convey. A highly frequent symbol (one which has a high probability of occurrence) conveys very little 'information', that is to say it is highly predictable. An infrequent symbol conversely conveys a great deal of 'information'. This may be expressed as the number of binary choices necessary to select the symbol by the formula
>
> $$H = -\log_2 p,$$
>
> where 'p' is the probability of the symbol. When this is done we may represent each frequency class by a figure showing the amount of 'information' which is attached to a member of that class.

Applying these principles to our present data the experimenter should complete the following table in which the frequencies of the different stimulus items have been transformed into 'bits of information'.

TABLE X

Paralog pairs	*Frequency in pre-test*	*Amount of information*	*Mean exposure time (or intensity) at recognition*
1	25	2·00 bits	
2	10	3·32 bits	
3	5	4·32 bits	
4	2	5·64 bits	
5	1	6·64 bits	

By plotting threshold against 'bits' the experimenter should now test the hypothesis that, according to the relationship we should expect to find in a communication channel, threshold values increase linearly with increases in the amount of information being transmitted.

DISCUSSION

In discussing his results the experimenter should examine carefully the four hypotheses upon which the research was based, drawing conclusions as far as he is able regarding the effect upon recognition of the different variables investigated.

He should furthermore consider his results in the context of the perceptual defence controversy, information theory, and the hypothesis of perceptual schemata.

Finally he should attempt a brief statement upon the nature of the recognition process.

GROUP EXPERIMENT

Where shortage of apparatus precludes individual experiments, the following procedure may be used. A group of subjects is seated facing a screen upon which, by rearward projection, words are presented at gradually increasing intensity. Above each word is positioned a line of dots varying in luminance from well below threshold to well above.

Within this situation the subject's first task is confined to recording the number of dots he can see at each increase in intensity of the verbal stimulus as signalled by the experimenter. His second task is to record pre-recognition guesses for each increment of intensity up to that level at which he recognizes each word correctly.

To control against order effects half the class should have one order and the other half another.

The subsequent matching procedure and treatment of results should emulate those outlined for the individual experiments.

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REFERENCES

General

ALLPORT, F. H. (1955). *Theories of Perception and the Concept of Structure*. New York: John Wiley.

ATTNEAVE, F. (1959). *Applications of Information Theory to Psychology*. New York: Holt.

BARTLETT, F. C. (1932). *Remembering*. Cambridge University Press.

BENDER, M. B., and TEUBER, H. L. (1946). *Arch. Neurol. Psychiat.*, **56**, 300.

BENDER, M. B., and TEUBER, H. L. (1949). *J. Psychol.*, **28**, 223.

BLUM, G. S. (1955). *J. abnorm. Soc. Psychol.*, **56**, 24.

BRENNER, M. (1956). *J. Person.*, **25**, 202.

BRENNER, M. (1957). *Quart. J. exp. Psychol.*, **9**, 169.

BROWN, J. F., and VOTH, A.C. (1937). *Amer. J. Psychol.*, **49**, 543.

BRUCE, D. (1955). 'Effects of context upon intelligibility of heard speech', ed. Cherry, Information Theory, London Symposium. New York: Academic Press, 1956.

BRUNSWIK, K. E. (1956). *Perception and the Representative Design of Psychological Experiments*. University of California Press.

CERMAK, P., and KOFFKA, K. (1922). *Psychol. Forsch.*, **1**, 66.

CHRISTMAN, R. J. (1953). *Amer. J. Psychol.*, **66**, 66.

DIMMICK, F. L., and SCAHILL, H. G. (1925). *Amer. J. Psychol.*, **36**, 412.

DIXON, N. F. (1956). *Int. J. Psycho.-Anal.*, **XXXVII**, 159.

DIXON, N. F. (1958). *J. abnorm. Soc. Psychol.*, **51**, 29.

DIXON, N. F. (1958a). *Quart. J. exp. Psychol.*, **10**, 211.

DIXON, N. F. (1958b). *Quart. J. exp. Psychol.*, **10**, 62.

DIXON, N. F. (1960). *Brit. J. Psychol.*, **51**, 4, 297.

ERIKSEN, C. W. (1960). *Psychol. Rev.*, **67**, 279.

FISHER, C. (1954). *J. Amer. Psychoanal. Ass.*, **11**, 389.

FRENKEL-BRUNSWIK, E. (1949). *J. Person.*, **18**, 108.

GENGERELLI, J. A. (1948). *J. exp. Psychol.*, **38**, 592.

GIBSON, J. J. (1950). *The Perception of the Visual World*. Boston: Houghton-Mifflin.

GILINSKY, A. S. (1955). *Amer. J. Psychol.*, **68**, 173.

GOLDSTEIN, M. J. (1960). *J. abnorm. Soc. Psychol.*, **60**, 22.

HAMILTON, V. (1960). *Brit. J. Psychol.*, **51**, (3), 257.

HELSON, H. (1943). *J. opt. Soc. Amer.*, **33**, 555.

HOCHBERG, C. B., and HOCHBERG, J. E. (1952). *J. Psychol.*, **34**, 107.

HOWES, D. H. (1954). *Psychol. Rev.*, **61**, 98.

HOWIE, D. (1952). *Psychol. Rev.*, **59**, 308.

ITTELSON, W. H. (1951). *Amer. J. Psychol.*, **64**, 56.

JONES, E. E., and BRUNER, J. S. (1954). *Brit. J. Psychol.*, **45**, (3), 157–65.

KANT, E. (1781). *Kritik der reinen Vernunft.*

KENDALL, M. G. (1948). *Rank Correlation Method.* London: Griffin.

KILPATRICK, F. P. (1955). 'Recent transactional perceptual research: a summary'. Final report on Office of Naval Research Contract N 60, no. 27014.

KING-ELLISON, P. (1953). 'Visual duration threshold as a function of word frequency: a replication'. Technical Report No. 6, University of Minnesota.

KLEIN, G. S. (1951a). 'The personal world through perception', in *Perception – An Approach to Personality*, ed. Blake, R. R., and Ramsey, G. V. New York: Ronald.

KLEIN, G. S., and SCHLESINGER, H. (1915b). *J. Person.*, **19**, 289.

KOFFKA, K. (1935). *Principles of Gestalt Psychology.* London: Kegan Paul.

KÖHLER, W. (1929). *Gestalt Psychology.* London: G. Bell.

KÖHLER, W., and WALLACH, H. (1944). *Proc. Amer. Philos. Soc.*, **88**, 269.

KORTE, A. (1919). *Beiträge zur Psychologie der Gestalt*, ed. Koffka, K.

LAZARUS, R. S. (1956). *Psychol. Rev.*, **63**, 343–7.

LAZARUS, R. S., and MCCLEARY, R. A. (1951). *Psychol. Rev.*, **58**, 113.

LIVSON, N. H. (1953). *Amer. J. Psychol.*, **66**, 365.

MACKAY, D. M. (1952). 'In search of basic symbols', in *Cybernetics.* New Jersey: Josiah Macy Jr. Foundation Publications.

MCKENNELL, A. C. (1960). *Brit. J. Psychol.*, **51**, 27.

MCGINNIES, E. (1949). *Psychol. Rev.*, **56**, 244.

MCGINNIES, E., and SHERMAN, H. (1952). *J. abnorm. Soc. Psychol.*, **47**, 81.

NEISSER, U. (1954). *J. exp. Psychol.*, **47**, 399.

OSGOOD, C. E. (1953). *Method and Theory in Experimental Psychology.* New York: Oxford University Press.

OSGOOD, C. E. (1957). 'Motivational dynamics of language behaviour', in *Nebraska Symposium on Motivation*, ed. Jones, M. R. University of Nebraska Press.

POSTMAN, L., BRUNER, J. S., and MCGINNIES, E. (1948). *J. abnorm. Soc. Psychol.*, **48**, 215.

SAMUELS, I. (1959). *Psychol. Bull.*, **56**, 1.

SHANNON, C. E., and WEAVER, W. (1949). *The Mathematical Theory of Communication.* Urbana: University of Illinois Press.

SIEGEL, S. (1956). *Non Parametric Statistics for the Behavioral Sciences.* New York: McGraw-Hill.

SOLOMON, R. L., and HOWES, D. H. (1951). *Psychol. Rev.*, **58**, 256.

SOLOMON, P., LEIDERMAN, P. H., MENDELSON, J., and WEXLER, D. (1957). *Amer. J. Psychiat.*, **114**, 357.

STEVENS, W. L. (1939). *Psychol. Rev.*, **46**, 142.

THORNDIKE, E. L., and LORGE, I. (1944). *The Teacher's Word Book of 30,000 Words.* New York: Bureau of Publications, Teachers' College, Columbia University.

TOCH, H. H., and ITTELSON, W. H. (1956). *Brit. J. Psychol.*, **47**, 195.

VERNON, M. D. (1952). *A Further Study of Visual Perception.* Cambridge University Press.

WERTHEIMER, M. (1912). *Zschr. Psychol.*, **61**, 161.

WHITTAKER, E., and GILCHRIST, J. C. (1952). *J. abnorm. Soc. Psychol.*, **47**, 732.

WOODWORTH, R. S. (1938). *Experimental Psychology.* New York: Holt.

Constancy

ADAMS, G. K. (1923). *Amer. J. Psychol.*, **34**, 359.

AMES, A. (1946a). 'Some demonstrations concerned with the origin and nature of our eye sensations', in *A Laboratory Manual.* Dartmouth Eye Inst.

AMES, A. (1946b). Supplementary notes.

BEVERIDGE, W. M. (1935). *Brit. J. Psychol.*, **26**, 1959.

BEYRL, F. (1936). *Zschr. Psychol.*, **100**, 344.

BOLLES, R. C., and BAILEY, D. E. (1956). *J. exp. Psychol.*, **51**, 222.

BORING, E. G. (1942). *Sensation and Perception in the History of Experimental Psychology.* New York: Appleton-Century.

BORING, E. G. (1943). *Amer. J. Physiol.*, **11**, 55.

BROWN, K. T. (1954). *Amer. J. Psychol.*, **67**, 533.

BRUNER, J. S., POSTMAN, L., and RODRIGUES, J. (1951). *Amer. J. Psychol.*, **64**, 216.

BRUNSWIK, E. (1929). *Zschr. Psychol.*, **109**, 40.

BRUNSWIK, E. (1944). *Psychol. Monog.*, **56**, No. 1.

BRUNSWIK, E. (1956). *Perception and the Representative Design of Psychological Experiments*. University of California Press.

BRUNSWIK, E., and CRUICKSHANK, R. M. (1937). *Psychol. Bull.*, **34**, 713.

BURKAMP, W. (1923). *Zschr. Sinnes Physiol.*, **55**, 133.

BURZLAFF, W. (1931). *Zschr. Psychol.*, **119**, 177.

BUYTENDIJK, F. J., and VAN RATINGEN, J. M. R. (1951). *Année Psychol.*, **50**, 371.

CRAMER, T. (1935). *Zschr. Sinnes Physiol.*, **54**, 215

DADOURIAN, H. M. (1946). *Amer. J. Physiol.*, **14**, 65.

DUKES, W. P. (1951). *Amer. J. Psychol.*, **64**, 87.

DUNCKER, K. (1939). *Amer. J. Psychol.*, **52**, 255.

EISSLER, K. (1933). *Arch. f. d. ges. Psychol.*, **88**, 487.

FISCHEL, H. (1926). *Zschr. Psychol.*, **98**, 342.

GELB, A. (1930). *Bethe's Handbuch norm. path. physiol.*, **12**, 1, 594.

GIBSON. J. J. (1950). *The Perception of the Visual World.* Boston: Houghton-Mifflin.

GIBSON, J. J., and GLASER, N. M. (1947). 'Motion picture testing and research'. Army Air Force Aviation Psychology Program Research Reports.

GILINSKY, A. S. (1951). *Psychol. Rev.*, **58**, 460.

GOGEL, W. C., HARTMAN, B. O., and HARKER, G. S. (1957). *Psychol. Monog.*, **71**, No. 422.

GOTTHEIL, E., and BITTERMAN, M. E. (1951). *Amer. J. Psychol.*, **64**, 406.

GOTZ, W. (1926). *Zschr. Psychol.*, **99**, 247.

GRANIT, A. R. (1926). *Skandin. Arch. f. Physiol.*, **64**, 406.

GREGORY, R. L., WALLACE, J. G., and CAMPBELL, F. W. (1959). *Quart. J. exp. Psychol.*, **11**, 54.

GRUBER, H. E. (1954). *Amer. J. Psychol.*, **67**, 411.

HASTORF, A. H. (1952). 'The influence of suggestion on the relationship between stimulus size and perceived distance', in *Human Behaviour from the Transactional Point of View*, ed. Kilpatrick. New Hampshire: Institute for Associated Research.

HEINEMANN, E. G., TULVING, G., and NACHMIAS, J. (1959). *Amer. J. Psychol.*, **72**, 32.

HELSON, H. (1938). *J. exp. Psychol.*, **23**, 439.

HELSON, H. (1943). *J. opt. Soc. Amer.*, **33**, 555.

HELSON, H. (1948). *Psychol. Rev.*, **55**, 297.

HENNEMAN, R. H. (1935). *Arch. Psychol.*, No. 179.

HERING, E. (1905). 'Zur Lehre Vom Lichtsinn'. *Gräfe-Sämische Haenbuch der gesante Augenheilkunde*, 1, 12. Leipzig.

HERMANS, T. G. (1954). *J. exp. Psychol.*, **48**, 204.

HOCHBERG, C. B., and HOCHBERG, J. E. (1952). *J. Psychol.*, **34**, 107.

HOLADAY, B. E. (1933). *Arch. f. d. ges. Psychol.*, **88**, 419.

HOLWAY, A. F., and BORING, E. G. (1940). *Amer. J. Psychol.*, **53**, 537.

HSIA, Y. (1943). *Arch. Psychol.*, N.Y., No. 284.

ITTELSON, W. H. (1952) 'Size as a cue to distance', in *Human Behavior from the Transactional Point of View*, ed. Kilpatrick. New Hampshire: Institute for Associated Research (1953).

ITTELSON, W. H., and KILPATRICK, F. P. (1952). 'The size-distance invariance hypothesis', ibid.

IZZET, T. (1934). *Archiv. f. d. ges. Psychol.*, **91**, 305.

JAENSCH, E. R., and MÜLLER, E. A. (1920). *Zschr. Psychol.*, **83**, 266.

JENKIN, N., and HYMAN, R. (1959). *Amer. J. Psychol.*, **72**, 68.

JOYNSON, R. B. (1949). *Quart. J. exp. Psychol.*, **1**, 119.

JOYNSON, R. B. (1958). *Quart. J. exp. Psychol.*, **10** (2), 65.

JOYNSON, R. B. (1960). *Quart. J. exp. Psychol.*, **10** (2), 142.

JOYNSON, R. B., and KIRK, N. S. (1960). *Quart. J. exp. Psychol.*, **12** (4), 221.

KARDOS, L. (1934). *Zschr. Psychol.*, Erg. Bd., **23**.

KATONA, G. (1929) *Psychol. Forsch.*, **12**, 94.

KATZ, D., and RÉVÉSZ, G. (1921). *Zschr. angew. Psychol.*, **18**, 307.

KLIMPFINGER, S. (1933). *Arch. f. d. ges. Psychol.*, **88**, 551.

KOFFKA, K. (1935). *Principles of Gestalt Psychology*. London: Kegan Paul.

KÖHLER, W. (1915). *Abh. Kgl. Preuss. Akad. Wiss.*, Phys. Math. Klasse, Nr. 3.

LEIBOWITZ, H., BUSSEY, T., and McGUIRE, P. (1957). *J. opt. Soc. Amer.*, **47**, 658.

LEIBOWITZ, H., and HARTMAN, T. (1959). *Science*, **130**, 569.

LOCKE, N. M. (1937). *J. gen. Psychol.*, **51**, 255.

LOCKE, N. M. (1938). *Psychol. Rev.*, **45**, 335.

MACLEOD, R. B. (1932). *Arch. Psychol.*, No. 135.

MACLEOD, R. B. (1940). *Science*, **92**, 604.

MACLEOD, R. B. (1947). *Miscellanea Psychologica Albert Michotte*, p. 138. Louvain and Paris.

MAKINO, T. (1950). *Jap. J. Psychol.*, **20** (2), 1.

MARTIN, T. M., and PICKFORD, R. W. (1938). *Brit. J. Psychol.*, **29**, 92.

McKENNELL, A. C. (1960). *Brit. J. Psychol.*, **51**, 27.

MOHRMANN, K. (1939). *Zschr. Psychol.*, **145**, 146.

OLDFIELD, R. C. (1960). *Quart. J. exp. Psychol.*, **12** (1), 65.

PIAGET (1945, 1946). *Arch. de Psychol.*, **29**, 255.

PIAGET and LAMBERCER (1943). *Arch. de Psychol.*, **29**, 173.

POSTMAN, L., BRONSON, W. C., and GROPPER, G. L. (1953). *J. abnorm. Soc. Psych.*, **48**, 215.

ROELFS, C. O., and ZEEMAN, W. P. C. (1957). *Ophthalmologica*, **133**, 188.

RUMP, E. E. (1961). *Brit. J. Psychol.*, **52** (2), 111.

SCHUR, E. (1926). *Psychol. Forsch.*, **7**, 44.

SHEEHAN, M. R. (1938). *Arch. Psychol.*, No. 222.

STAVRIANOS, B. K. (1945). *Arch. Psychol.*, No. 296.

STEVENS, S. S. (1951). *Handbook of Experimental Psychology*. London: Chapman & Hall.

STEWART, E. C. (1959). *J. exp. Psychol.*, **57**, 235.

TAYLOR, D. W., and BORING, E. G. (1942). *Amer. J. Psychol.*, **55**, 189.

TAYLOR, J. G., and PAPERT, S. (1956). *Brit. J. Psychol.*, **47**, 216.

THOULESS, R. H. (1931a). *Brit. J. Psychol.*, **21**, 339.

THOULESS, R. H. (1931b). *Brit. J. Psychol.*, **22**, 1.

THOULESS, R. H. (1932). *Brit. J. Psychol.*, **22**, 216.

UEXKÜLL, J. B., and KRISZAT, G. (1934). *Streifzüge durch die Umwelten von Tieren und Menschen*. Berlin: Springer.

VERNON, M. D. (1937). *Visual Perception*. Cambridge University Press.

VERNON, M. D. (1952). *A Further Study of Visual Perception*. Cambridge University Press.

VERNON, M. D. (1957). *Brit. J. Psychol.*, **48**, 35.

WALLACH, H. (1948). *J. exp. Psychol.*, **38**, 310.

WALLACH, H., and GALLOWAY, A. (1946). *J. exp. Psychol.*, **36**, 119.

WEBER, C. O., and BICKNALL, N. (1935). *Amer. J. Psychol.*, **47**, 436.

WHITESIDE, T. C. D., and CAMPBELL, F. W. (1959). *Quart. J. exp. Psychol.*, 11 (4), 249.

WOODBURNE, D. L. S. (1934). *Amer. J. Psychol.*, **46**, 273.

WOODWORTH, R. S. (1938). *Experimental Psychology*. New York: Holt.

Chapter 3

REMEMBERING

IAN M. L. HUNTER

Keele University,[*] Staffordshire

The territory to be explored in this chapter is remembering, that is, the mode of ingress of a person's past into his present activity. This territory has not been encharted exhaustively, but even if it had it would still be contrary to the purpose of this book to lay the chart before the reader. (A non-technical overview is given by Hunter (1957), and further references will be given at the end of the chapter.) Nor, in the compass of this chapter, would it be possible to guide anybody's steps to all the known highways and byways in the area of remembering. Only a few pointers can be given in the hope of stimulating the user of this book to make his own first-hand observations of psychological activities and to develop his appreciation that these activities comprise a fitting subject matter for adventurous and rewarding empirical study. However, before setting out on these explorations, it might be well to glance briefly at the main achievements of those psychologists whose studies first penetrated into this area.

HISTORICAL OVERVIEW

Comparing mind[†] to a storehouse, learning involves the storing away of perceived events, and remembering the bringing of them out of storage for use in the present. It was in the spirit of this analogy that Ebbinghaus (1850–1909) made his pioneering attack. At the present time, it is easy to overlook the bold originality embodied in Ebbinghaus' book (1885) which amounted to nothing less than the first systematic experimental study of the higher mental processes. Ebbinghaus' approach was akin to that of the physical scientist in its use of strictly controlled experimental situations and quantitative treatment of results. He committed lists of material to memory and, after varying intervals of time, found out how well he could remember what he had memorized: he achieved experimental control by standardizing the learning procedure (the anticipation method), the material to be learned (the nonsense syllable), the remembering procedure, and the quantitative assessment of how much had been remembered or forgotten (the saving score). His final contribution was twofold. First, he invented methodological tools which have stood psychologists in good stead ever since. (Some of these will be used in Exp. II. Hilgard (1951) gives detailed descriptions of them and useful lists of nonsense syllables and other verbal material.) Secondly, he employed his experimental techniques to make a number of factual discoveries; some concerned memorizing and others, notably his 'forgetting curve', remembering. His success prompted later psychologists to work within the tradition he established; the outcome is a body of knowledge unsurpassed in contemporary psychology for its precision and lawfulness.

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[*] Professor Hunter was formerly at Edinburgh University.

[†] Mind is a concept about which there has been, and still is, much controversy. In keeping with the empirical approach of this chapter, 'mind' is used here to refer collectively to all those processes which determine the higher levels of organization in behaviour. In this sense, mind concerns bodily processes and, furthermore, our knowledge of mind is chiefly inferential. A brief and easily readable discussion of this usage of 'mind' is given by Hebb (1958), pp. 2–5.

However, the path blazed by Ebbinghaus left large tracts of remembering unexplored. It is the advantage of experimental procedures that they standardize and control the circumstances under which phenomena are observed: it is one of their dangers that they so restrict the phenomena as to deny them full characteristic expression. Remembering does not, for the most part, manifest itself in the recalling of memorized nonsense syllable lists but of meaningful past experiences. Accordingly, Bartlett (1932) studied the remembering of material more representative of 'real-life'. For example, he gave people a short story to read and later asked them to recount it. In doing this, he sacrificed some of the precision of the Ebbinghaus approach and the interest of his results lay less in the amount remembered than in its qualitative distortions. (For records of such recall performances see Bartlett (1932) and Hunter (1957).) Apart from introducing a more 'real-life' experimental approach, this work revealed the inadequacy of any simple storehouse conception of mind. The person does not passively record the events around him but selects from among them, interprets them, and modifies them in accordance with his enduring interests and momentary attitudes and expectations; nor does he remember by bringing these assimilated events out of cold storage but, again, by selecting from among them, piecing them together, and modifying them in accordance with attitude. 'Remembering is not the re-excitation of innumerable fixed, lifeless and fragmentary traces. It is an imaginary reconstruction, or construction, built out of the relation of our attitude towards a whole active mass of past reactions or experience . . .' (Bartlett 1932, p. 213). In addition, it is not only in storing them and in unstoring them that the person selects and transforms events; in the intervening interval also, the contents of the storehouse are constantly being rearranged under the impact of the person's ongoing activities (see Exp. II).

A further explorer who broke away from the static storehouse concept of mind was Koffka (1886–1941). Subsequent work has shown that his sojourns into the area of remembering (see Koffka 1935, chaps. X and XI) traversed slippery and uncertain ground (see Osgood 1953, chap. 13, for a critique); nevertheless, his journeyings forced recognition of two broad and basic principles.

1. Mind is unitary. It is difficult to isolate 'pure' mental functions since they hardly ever, if at all, exist as such. In fact perceiving, learning, remembering, thinking, and so on are best regarded as terms descriptive of *aspects* of a complex and unitary psychological activity, much as volume and weight describe aspects of physical objects.

2. Mind is dynamic. It is in a continuous state of change and flux. Remembering, for example, is one aspect of ongoing motivated activity, an aspect whose nature and very occurrence depend on this activity. It is this fact which is being asserted when we examine the 'effect of set on remembering' (see Exp. III) and it should be pointed out in passing that one of the most dramatic discoveries concerning this effect was made by Freud (1856–1939). His observations were carried through not under experimental conditions which simulated 'real-life' but in the medical treatment of neurotic patients; his discovery, through prolonged naturalistic observation, of repressing shows the selective nature of remembering at its most impressive, shows the potent effect of a set *not* to remember.

OBSERVATIONS ON CONTINUOUS CONTROLLED RECALL

This simple experiment is conducted by a single person who acts as both subject and experimenter. It is best done in solitude with about twenty minutes devoted to the remembering activity.

Procedure. The person supplies himself with paper, pencil and a watch with a good seconds hand. He decides on any one letter of the alphabet, say *D*, and attempts to recall as many words as he can, each beginning with this letter. As the words are recalled, they are written down in a column and, at the end of each successive minute, a horizontal line is drawn beneath the most recently recalled word. This provides a record of the words recalled, their order of recall, and the number recalled in each minute period. The procedure is followed for some twenty minutes. *It should be performed before reading the discussion below.*

Results. Examination of the record will prompt a host of questions and rapidly convince one of the complexity of this performance, the most obvious characteristics of which are as follows.

Performance is not temporally regular. Words do not emerge in a steady stream, e.g. one every five seconds, but in irregular bursts.

Performance is not confined to 'pure' recall. Quite apart from the activity of keeping the time check, the subject engages in a variety of perceptual and thinking activities. He looks round the room 'for inspiration', reasons in terms of alphabetical sequences (see ch. 6, below), wonders whether this or that word is appropriate, whether he recalled it earlier on, whether it is a word at all.

Words are not typically recalled at random. While it may be said that the subject must have a 'store' of, for example, *D* words, it may certainly not be said that he draws from this 'store' in the random manner of a blindfolded person drawing numbered tickets from a jar. There are, in the record, many instances of 'clustering', that is, of runs of words which are similar to each other in their meaning, sound, or in some other way. The activity of recalling an item facilitates the recall of another item which is related to it (*relatedness increment*) and reflects the fact that knowledge is made available in an organized rather than a piecemeal fashion. (Elaborate statistical demonstrations of relatedness increment have been given by Bousfield: his 1953 paper details some of this work. When people are having difficulty in recalling a person's name and they try to help themselves by recalling as much as they can about this person – his appearance, where they last met, his occupation, his home, etc. – they are exploiting relatedness increment.) Clustering may occur without the subject's being aware of it at the time: more often, the subject is aware that he is producing words of a related kind and that:

Performance has characteristics of a search. He deliberately sets himself to recall items now of this, now of that category. For example, he tries to recall the names of girls, then of towns, of colleagues, flowers, Shakespearean characters, animals. He may also set himself to work systematically through the alphabet (dab, dac, dad, . . . deb, dec, ded, . . . dib, dic, did . . .) thereby producing syllables and sounds which may then be either recognized as words in themselves or prompt his recalling of related words. (This illustrates the intimate interaction of reasoning and recalling and also of recognizing and recalling. Remembering often involves reasoning, e.g. a man 'remembers' that, last year, his wife's birthday must have fallen on a Sunday because he recalls that they could not celebrate by going to the theatre. An instance of the interaction of recognizing and recalling occurs where, in trying to remember a name, people recall only some general characteristic of it – it was French-sounding, or short and effeminate – and then proceed to recall names having this general characteristic until one is recalled which is recognized as being correct.) Thus, at intervals throughout his performance, the subject concentrates on one particular 'region of search', is set to recall words of one particular kind. Such sets show their effects not only in the subject's private awareness but also in the runs of related words in the record and the sudden increases in the speed of recall. Where words are recalled in a rush, it is usually because a new 'region' is being 'searched'. For example, in one performance the stimulus letter was *D* and the number of words recalled in successive two-minute intervals was 22, 9, 8, 17, 7, 4, 13, 16, 9 and 13. At first there was a burst of unrelated words followed by a run of girls' names and then rhyming words. After that came a few unrelated words interspersed by short unproductive periods. The increase in the fourth period was due to the sequence 'doh, doe, do, did, does, doer, done' followed by a run of names of towns. The next increase came in the seventh and eighth periods with a spate of words all formed by using the prefix 'dis-'. The last spurt involved a run of names of psychologists. From this example alone, 'regions of search' are clearly evident.

Comment on naturalistic and experimental observation. Naturalistic observation simply means the noting of events as they occur 'spontaneously' in nature. It is one with so-called common sense but must not, for that reason, be despised. It is the starting point of scientific exploration and, with interested and experienced students, the observations may be acute and highly sophisticated inferences may be drawn – witness the work of Darwin in zoology and Freud in psychology. The above discussion of continuous controlled recall was based on procedures which involved little else than naturalistic observation. There often comes a point, however, at which the observation of 'naturally' occurring events must be supplemented by that of events over which the student can exercise some control. Experiment becomes necessary in order to provide those particular observations which, for a variety of reasons, are difficult or impossible to obtain in 'nature' and yet are necessary for the verification and elaboration of inferences which have, in the first place, been drawn from naturalistic observation. Furthermore, it often happens that, because of the adventurous and fortuitous nature of research activity, such experiments lead to illumination of phenomena far removed from those involved in the initial naturalistic observations. The study of continuous controlled recalling of words is a case in point. Observations of the kind made above suggested characteristics which psychologists then elucidated and quantified by the statistical analysis of data obtained experimentally from large numbers of subjects. Conclusions emerged which were of interest to students of remembering activity – and also, and unexpectedly, to students of problem-solving activity (see Hunter 1959).

The circumstance that experimental observation frequently takes place within the walls of a laboratory and with the aid of much apparatus, gadgetry, and 'know-how', should not obscure the fact that it is a refinement and an extension of naturalistic observation and is not basically different from it. The experiment now to be performed is designed for use with groups of university undergraduates, takes about half an hour to introduce and perform, and is included less for the psychological insights it affords than for the experimental problems it poses.

EXPERIMENT I: REMEMBERING BY RECALL AND BY RECOGNITION

Purpose. To compare the amount recalled with the amount recognized.

Materials. Two learning-lists, One and Two, each comprising ten three-letter nonsense syllables. Also, for each learning-list, a recognition-list of thirty three-letter syllables in which the ten original items are interspersed among twenty new items.* Stop-watch.

General procedure. The subject is allowed enough time for a partial memorizing of the learning-list and his remembering is studied under one of two alternative conditions. That is, he either writes out as many of those items as he can (recall); or he attempts to select the items from those contained in the recognition-list (recognition). The difference between the recall and recognition performances is the variable to be studied and so all other variables must be kept as constant as possible.

Difficulties. The main difficulty for the experiment is that, after any one person has memorized any one list, he cannot be regarded as remembering by *both* recall and recognition: at the least, there would be a time difference between his two performances. Comparisons must therefore be made between different persons, some of whom attempt recall and some recognition. The procedure may be schematized as follows.

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* The three experiments described in this chapter require the use of carefully compiled material and also demand that this material should not be read beforehand by those taking part in the experiments. This material comprises eleven lists: two learning and two recognition lists for Experiment I, four learning-lists (two of which are identical) for Experiment II, and three anagram arrays for Experiment III. Copies of suitable material (2*s.* 6*d.*) printed ready for use are available from the publisher, to *instructors only*. The instructor will provide copies which must be returned to him.

Subject 1. Learns lesson X . . . time interval . . . Recalls X.
Subject 2. Learns lesson X . . . time interval . . . Recognizes X.

This comparison now raises further difficulty due to individual differences between subjects and, in this experiment, these differences can be counterbalanced only by using a large number of subjects, by comparing two groups of people rather than two individuals. There are, of course, a number of variables which can, to some extent, be made uniform from one subject to the next. The most obvious of these are: the list memorized; the time allowed for memorizing; the time elapsing between the removal of the learning-list and the start of the remembering performance; the time allowed for remembering; the subject's method of approaching the memorizing task; and the subject's fore-knowledge of the circumstances of his own remembering performance. With regard to the last point, it could be that a subject's remembering would be improved if he had known, at the time of memorizing, whether his remembering was to be by recall or by recognition. Thus, while the subject knows beforehand that he is either to recall or recognize, he should not know in advance which of these performances is to be asked of him. (The effect, if any, of such fore-knowledge could be discovered by conducting a further experiment.*)

Of the several ways in which the above general procedure may be put into practice, two are described below. The Class Experiment meets the requirements of a properly controlled experiment but demands the combination of results from several subjects; the more taking part, the better. When less than about eight are available, the use of the Individual Experiment is recommended. This is incomplete as regards experimental design but, granted the findings of a well conducted Class Experiment, it demonstrates, for each individual, the difference between recall and recognition scores.

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* Such an experiment was conducted with 160 students acting as subjects. Half were told, before memorizing, whether they would subsequently recall or recognize: half were not told until after memorizing. The amount remembered by each of the two groups was identical. Thus, in the present situation, prior knowledge of the nature of the remembering task has no effect on the performance of this task.

EXPERIMENT I: CLASS EXPERIMENT

Procedure. The experiment is done by persons working in pairs and each pair is provided with four lists, i.e. learning-lists One and Two, and recognition-lists One and Two. The class instructor describes the procedure as follows.

The experiment is to be conducted in two parts. In the first, one person will act as experimenter, the other as subject. In the second, roles are exchanged and the remaining learning-list used. In each part, the experimenter will cover whichever learning-list is to be used, place it before the subject and, on a signal from the instructor, uncover the list: whereupon the subject will attempt to memorize the spellings (rather than the pronunciations) of the ten syllables by silently reading the list again and again from beginning to end until told to stop. After exactly sixty seconds, the instructor will signal the experimenter to remove the list. The instructor will then ask subjects in one half of the room to recall and the remaining subjects to recognize. It will not be until he has completed his attempt at memorizing that a subject will know whether he is to recall or recognize. In recall, the subject will try to write out 'from memory' as many of the ten syllables as possible. In recognition, the subject will consult the appropriate recognition-list and (without marking the list) *copy out* from it those syllables which he recognizes as having been in the learning-list. In both recall and recognition, the aim is to write out as many of the original syllables as possible (and only the originals) and the order in which they are written is irrelevant.

Some pairs of persons will start by having the first subject memorize learning-list One while some will have the first subject memorize Two. *The first experimenter in each pair should avoid as far as possible reading the syllables learned and remembered by his subject;* otherwise these syllables will confuse him when, in his turn, he comes to deal with the alternative list. It is important *that neither subject should read through his learning- or recognition-list before performing the experiment.*

The above instructions are read aloud by the instructor and questions invited. When everybody understands them, half of the experimenters are asked to prepare learning-list One for whole presentation and half to prepare learning-list Two. The first part of the experiment is then performed, subjects being given about two minutes for remembering. Remembering performance is not scored at this stage: roles are reversed and the second part of the experiment performed with the remaining learning-list.

Treatment of results. The quantitative assessment of recognition performance is complicated by the fact that a subject often selects one or more of the new items as having occurred in the learning-list. For want of a better name, it can be said that these erroneous selections occur by guesswork. Now if a subject selects new items by guessing, may he not also select some of the 'old' items in this way, rather than by genuine recognition? For any particular subject this question cannot be answered with any certainty either by the experimenter or by the subject himself. But by guessing alone the statistical chances are that a subject will select one old item for every two new items, the ratio of old to new items being one-to-two in the recognition-list. Accordingly, the recognition score is calculated by a method which, in terms of probabilities, is most likely to deal fairly with the effects of guesswork. The number of old items selected is reduced by subtracting one half of the number of new items selected. (For example, a selection of 9 old items and one new item gives a score of 8·5 items recognized; a selection of 8 old and 2 new items gives a score of 7.) Notice that, with this scoring system, a subject who selects ten items at random might sometimes make a positive recognition score and sometimes a negative score but that the score he is most likely to make is zero: where only guessing is involved, this is as it should be.

The recall score is calculated by totalling the number of syllables correctly written down. It might be thought that this total should be corrected in a manner comparable to that just mentioned for the recognition score since, on rare instances, a subject may ostensibly 'recall' a syllable not present in his learning-list. To make such a correction, the question would have to be asked: what are the subject's chances of guessing one of the ten old syllables? In the alphabet, there are 21 consonants and 5 vowels, so his guess would combine one of these consonants with one of the 5 vowels and one of the remaining 20 consonants. The number of syllables which may be constructed is obviously large, and, in making his guess, it is from this large number of possible syllables that the subject would, as it were, select. This being so, his chances of guessing one of the syllables in the learning-list are extremely small and any correction would be negligible. The interested person might care to test the preceding argument by having someone guess ten syllables and seeing how many of these correspond to a prepared list.

To sum up: the recognition score is the number of old items selected less half the number of new items selected. The recall score is the number of old items written down.

The general principles. When several recall and recognition scores are collected, it is generally found that the recognition scores are the higher. This illustrates the general principle. *Other things being equal and under the very great majority of conditions, the amount recognized is greater than the amount recalled.* This holds not only for nonsense syllables but for experienced events of all kinds, for melodies, touch sensations, visual scenes, and so on. In everyday life, the principle can often be observed in operation. People commonly remark that they cannot remember (i.e. recall) a name but would know (i.e. recognize) it if they saw or heard it. People also remark that they are poor at remembering names but never forget a face and, usually, they mean that they recall names more poorly than they recognize faces. (Experiment has shown that names can be more easily remembered than faces when names are to be recognized and faces recalled, but not when names are to be recalled and faces recognized.) British people engaged in learning French find it easier to give the English equivalent of a French word or phrase than to give the French equivalent of an English one. And children in the early stages of learning their first language (and quite apart from any motor difficulties) often understand a word spoken to them before they can say it. As a last example, the difference between 'new-type' and 'traditional' examinations may be mentioned. In the former, candidates must select the correct answer from among a given number of alternatives and their final (recognition) score is typically higher than in the traditional examination where the correct answer must be recalled.

Specimen results. The experiment described above was performed with several classes of university undergraduates and results obtained from a total of 200 subjects: 100 provided recall scores, 100 recognition scores. Since the results for lists One and Two were almost identical, the scores made on these two lists have been pooled. In both recall and recognition, the possible score ranges from zero to 10; the number of subjects making each score is shown in Table XI. These results show three noteworthy features.

TABLE XI

Score	0 or 0·5	1 or 1·5	2 or 2·5	3 or 3·5	4 or 4·5	5 or 5·5	6 or 6·5	7 or 7·5	8 or 8·5	9 or 9·5	10
Recog. (N = 100)	0	0	0	0	2	4	4	19	22	19	30
Recall (N = 100)	0	3	4	9	15	16	17	20	10	4	2

1. The two arrays of scores overlap. This means that the psychological performances represented by these scores are influenced by factors other than that selected by the experimenter as the critical variable. This illustrates two important points. First, psychological experiments are peculiarly difficult to control for, when using human beings, it is not possible to take into account every detail of their previous histories. Secondly, with data from only a few subjects results may be obtained which contradict the conclusions to be drawn from a larger sample. For example, a group of only four subjects might contain two who recall 10 syllables and two who recognize only 4 syllables. This possibility illustrates the fallacy of generalizing from too few instances.

2. The recognition scores show a 'ceiling-effect', the scores bunching up in the highest possible category. This means that, had circumstances permitted, some subjects could have scored higher than 10. This ceiling-effect could be removed if the experiment were redesigned either by allotting a shorter time (say 45 seconds) to the memorizing task or by presenting more syllables (say 14) to be memorized in 60 seconds.

3. The recognition scores are, on the whole, conspicuously higher than the recall scores, the averages being 8·50 and 5·56 respectively. Notice however that the difference between these averages would have been even greater but for the ceiling-effect: some subjects with a recognition score of 10 would have scored higher and so have raised the average score.

Use of probability in assessing results. Even with two sets of scores which, as here, appear to the eye as being distinctly different, it is desirable to state the extent of this difference in precise terms. This is done by stating the probability that the obtained difference could have arisen merely by chance. With data of the kind afforded by the present experiment, this way of stating the amount of difference would usually be arrived at by the procedure known as calculating a t-score* based on the averages and the standard errors of the two sets of scores. But properly speaking, a t-score can only be used when each of the arrays is normally distributed. So because of the ceiling-effect, a t-score is not appropriate here. However, an alternative statistical approach is available for the present data: this is the calculation of a chi square value.

Consider all 200 scores together. The average score lies between 7·5 and 8. Call any subject who scores 8 or more a 'high-scorer' and any who scores less than 8 a 'low-scorer'. In recall, the proportion of 'high' to 'low' scorers is 16 : 84, and in recognition 71 : 29. On calculation, the chi square value of these two proportions is found to be 59·32. In terms of probabilities, this value means that not once in a thousand repetitions of this experiment would these two proportions arise by chance and that there is a very significantly greater proportion of 'high-scorers' among the recognizers than among the recallers. In short, if it is assumed (as it may be under present conditions) that there existed no overall differences in the learning and remembering *abilities* of the recallers, *as a group*, and the recognizers, *as a group*, then it can confidently be concluded that more is remembered by recognition than by recall.

The conclusion is: *other things equal, recognition score is higher than recall score.* To the beginning student of psychology, this conclusion may seem so platitudinous as to be unworthy of the experimental labour involved in establishing it. If so, three comments are in order. First, the present experiment is an exercise in experimental exploration designed to illustrate procedures of psychological analysis rather than to lead to unexpected conclusions: to arrive at the unexpected usually requires experimentation of a kind which uses these same procedures in a more complex way. Secondly, an experimentally-based conclusion must not be despised because it seems 'obvious'; psychologists are all too familiar with 'self-evident' tenets which, on examination, are unsupported by fact. Thirdly, a conclusion which is 'commonsensical' must have been arrived at, however informally, on the basis of evidence of some sort: in the light of the present experiment, what sort of experiences could have given rise to a 'commonsensical' elaboration of the present conclusion?

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* See p. 23.

EXPERIMENT I: INDIVIDUAL EXPERIMENT

In this experiment, the memorizing, recalling and recognizing procedures are as before except that each subject attempts both recall and recognition. First, he is given a learning-list to memorize (for reasons to be given below, a time limit of 45 seconds rather than 60 seconds is recommended). Then he tries to recall as many of the syllables as he can and, immediately after this, attempts recognition. Using a 60-second memorizing period, the following results were obtained from 16 student subjects. Each subject is represented by a letter of the alphabet; his recall score and subsequent recognition score are shown; and '+' signifies that the latter is higher than the former. Twelve subjects score higher in recognition than in recall; of the remaining four, only one scores lower. But notice that these four subjects all make relatively high recall scores. Once more, there is a ceiling-effect since the higher the recall score the less scope there is for subsequent improvement in recognition. (This is why it was suggested that a relatively low recall score be ensured by reducing the memorizing time to 45 seconds.)

TABLE XII

Subject	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p
Recall	1	2	2	3	3	4	4	4	5	6	7	7	7	8	8	10
Recog.	7	7	7·5	4	7	7·5	8	10	9	9	9	7	6	8	10	10
	+	+	+	+	+	+	+	+	+	+	+				+	

The conclusion is: *when an individual's relatively poor recall performance is followed immediately by attempted recognition, his second remembering score is higher than his first.*

Some problems. It will be noticed that the conclusions drawn from the Individual and from the Class experiments are not identical. The user of this book will find it worthwhile to consider the exact difference between the two conclusions and just what evidence would be needed to bridge this difference. Suffice it to say here that when two successive remembering performances are attempted immediately after memorizing, the later performance would not be consistently better than the earlier if (*a*) both were by recognition, (*b*) both were by recall, or (*c*) the earlier were by recognition and the later by recall. In itself, condition (*c*) raises scoring problems for, on occasion, the subject will recall some of the 'new' items from the recognition list. How are such intrusions to be treated? Perhaps the reader might begin his search for an answer by planning and carrying out an experiment.

EXPERIMENT II: RETROACTIVE INTERFERENCE AND REMEMBERING BY SAVING

Purpose. To compare the effects of different interpolated learning activities on amount remembered as measured by a saving score.

Materials. Three learning-lists.* List One comprises eight three-letter nonsense syllables. List Two S is similar to List One for it also comprises eight (different) three-letter nonsense syllables. List Two D is dissimilar to List One, comprising thirteen two-syllable adjectives. A movable aperture, that is, a piece of paper or board with a hole just large enough to expose a single item while other items remain covered. (Such a movable aperture is simple to make: the alternative is to print each item on a separate card and present these cards one by one.) A metronome.

General procedure. Two groups of subjects are compared. Each subject learns a lesson until a certain degree of proficiency is reached. He then learns a second (interpolated) lesson and, after this, relearns the original lesson until he regains his former level of proficiency. The less he has forgotten of the first lesson, the more easily will he regain his earlier proficiency. For one group of subjects, the interpolated lesson is relatively similar to the original lesson whereas, for the other group, it is relatively dissimilar. The question is: will the two groups regain their earlier level of proficiency with equal ease?

Procedure. The procedure to be described has been successfully employed with groups of university undergraduates. The persons work in pairs, one as experimenter and one as subject. Each pair is provided with two learning-lists; half of the pairs with Lists One and Two S, the other half with Lists One and Two D. Although not essential, it is preferable that subjects should not know the purpose of the experiment before taking part in it. The instructor describes the procedure as follows.

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* For the material to be used, see p. 23.

The experiment involves three learning sessions. In the first the subject will memorize List One by the method of anticipation until a learning criterion of one errorless trial has been reached. The experimenter will record the number of trials required to reach this criterion. In the second learning session, the subject will memorize List Two. Again, learning will be by the method of anticipation and, again, the experimenter will record the number of trials required to reach one errorless trial. In the third session, List One will be relearned until, once again, the one trial criterion is reached and, as before, the number of trials required is recorded.

The anticipation method. The experimenter will place the list in front of his subject and cover it with the frame of the movable aperture. He conducts each trial by exposing, first, the list heading and thereafter each successive item in the list, each exposure lasting for 3 seconds. On the first trial, the subject will observe and attempt to memorize each item as it is exposed. Then, starting with the second run through, he will try to anticipate, during each exposure, the item which is to appear next. Thus, when the list heading is exposed, he attempts to call out the first item on the list; three seconds later, this first item will be exposed and he will attempt to call out the second item; and so on until the trial ends with the exposure of the last item. (In this, as in all class experiments, due consideration must be paid to persons working within earshot.) The experimenter will expose each item for exactly 3 seconds and will not wait for the subject to 'make up his mind' about the next item: it is part and parcel of the performance which the subject is required to master that he correctly anticipate each item during the exposure of the immediately preceding item. Note that each exposure will serve a dual purpose. Except for the exposure of the list heading, each confirms or corrects the anticipation just voiced; and except for the last in the list, each serves as a signal for anticipating the succeeding item, that is, prompts the subject in recalling the next item.

The list is gone through, trial after trial, until that trial is reached in which the subject anticipates each of the items correctly and at the appropriate time, that is, until he reaches a criterion of one errorless trial. Two aspects of this criterion are noteworthy. First, it is arbitrary. It would be just as admissible to decide on a criterion of two or more errorless trials. It merely happens that a one trial criterion is convenient for present purposes. Secondly, if, say, a subject reaches the criterion on his tenth trial, he would not necessarily reach a criterion of two consecutive errorless trials on his eleventh. The variability of learning performance is such that he would be likely to make some errors on this eleventh trial and not reach the two trial criterion until, say, his thirteenth trial or beyond.

The subject's memorizing performance will be assessed in terms of the number of trials to reach the criterion. This number will include the first trial, on which no anticipation is attempted, and also the criterion trial.

Timing. The timing must be exactly controlled. Each item should be exposed for precisely 3 seconds. This can be achieved by the experimenter's moving the aperture to the next item on every ring of a metronome bell set to sound twenty times per minute. The time between the end of one trial and the start of the next should also be constant and 12 seconds is a convenient interval. This inter-trial interval applies not only within each learning session but also between the end of one learning session and the start of the next. During this 12-second delay, the subject should resist the temptation to recite over to himself the contents of any of the lists.

Mode of anticipation. When learning words, the subjects will anticipate by pronouncing the word. When learning syllables, he will spell out the syllable. This spelling out has the advantages of avoiding ambiguities inherent in pronouncing certain syllables and (in the present experiment) accentuates the dissimilarity between the syllable and adjective lists.

Treatment of results. Each subject's performance furnishes three basic scores, namely, the number of trials to learn List One, to learn List Two, and to relearn List One. The saving score is calculated from the first and last of these three. The notion underlying the saving score is that of work expenditure. In learning a lesson, so much effort is expended and the number of trials required for learning is taken as a quantitative index of this effort. If what is learned is forgotten completely, then on relearning the same lesson, the same amount of effort will have to be expended as before. In terms of work, the person will be no better off than if he had never learned the lesson and his previous effort will not save him any effort now. If, on the other hand, he forgets nothing of what he learned, he need expend no effort in relearning; he will have saved himself all (100 per cent) learning effort. These considerations raise two important characteristics of the saving score.

1. *It ranges from zero to 100 per cent* and, in order that it may do so, the criterion trial (or trials) must be excluded from the learning and relearning scores. Suppose that 10 trials are required for original learning up to and including one errorless trial and that, on relearning, an errorless trial is achieved first run through. If the criterion trial were included, the saving score would be 9 (10 minus 1) divided by 10. But clearly the score should be 100 per cent since no effort is expended on relearning. However, if the criterion trial is excluded, the saving score is (9 minus 0) divided by 9, that is, 100 per cent – as it should be. In short, the percentage saving score is given by the following formula where 'x' is the number of trials to learn *exclusive of the criterion trial* and 'y' is the number of trials to relearn *exclusive of the criterion trial:* $100.(x - y)/x$.

2. *It depends on learning ability as well as amount retained.* Like any measure of remembering performance, whether by recall or by recognition, the saving score is affected by conditions operating at the time of remembering. So if, for any reason, the person's learning ability changes markedly between the time of learning and the time of relearning, this change ought to be allowed for in the final saving score. To illustrate, Ebbinghaus discovered that because of diurnal variations it took him an average of 12 per cent more trials to learn a syllable list at 6 to 8 p.m. than at 10 to 11 a.m. Thus, in determining the amount of forgetting during a nine-hour interval, the original learning took place at a favourable time of day and the relearning at an unfavourable time. Accordingly, he deducted 12 per cent from the relearning time before calculating the amount of saving. However, under the conditions of the present experiment, any change in learning ability between the times of learning and relearning (*a*) is slight and, on the whole, limited to a small beneficial practice effect, and (*b*) influences, in much the same way, both those subjects whose

interpolated activity involves learning List Two S and those whose activity involves learning List Two D. Thus, the overall change in ability invalidates only slightly, if at all, the inter-group comparison which is the experiment's objective. The relative stability of subject's learning abilities throughout the experiment does not, however, mean that an occasional subject's abilities may not change markedly for the worse through his becoming emotionally upset as the experiment proceeds. In such a case, he may require more trials to relearn the lesson than he originally required to learn it, that is, his saving score may be negative.

The principle. Retroactive interference refers to the fact that remembering performance depends not only on the original learning activity and on conditions operative at the time of remembering but also on the nature of the activities engaged in during the interval between learning and remembering. From a practical viewpoint, such interference is important for it is the most potent single factor responsible for forgetting and misremembering. From a theoretical viewpoint, retroactive interference illustrates the temporal unity of mind, the rule that psychological activities are interrelated in sequences and can be split only arbitrarily into discrete temporal units: in this respect, mind is historical, developmental, analogous to a continually flowing and turbulent stream.

When each subject's saving score has been calculated, it is found that, on the whole, the greater amount of saving is shown by those subjects whose interpolated activity is the more dissimilar to the original learning activity. This finding illustrates the role of retroactive interference in producing forgetting. Supplementary evidence for inter-lesson interference comes from at least two other aspects of the performances. (1) Intrusions: in relearning List One, a subject sometimes anticipates an item belonging to the interpolated List Two S but intrusions between Lists One and Two D do not occur. (2) On their first relearning trial, subjects in the Dissimilar Group correctly anticipate, on the whole, more items than do subjects in the Similar Group. The number of such correct anticipations gives a (prompted) recall score and the inter-group difference with respect to this score illustrates that retroactive interference affects remembering in general and not merely remembering as manifested in saving.

Specimen results. The experiment described above was performed with a total of 120 university undergraduates taking part. Sixty were subjects and half of these had, as their interpolated task, the learning of List Two S: these constitute the Similar Group. The remaining 30 subjects belonged to the Dissimilar Group. The saving scores of the Dissimilar Group ranged from 40 to 88 per cent with a mean of 74 per cent. The scores of the Similar Group ranged from −11 to 76 per cent with a mean of 46 per cent. Table XIII shows the number of subjects scoring less than 20 per cent, from 21 to 40 per cent, from 41 to 60 per cent, from 61 to 80 per cent and from 81 to 100 per cent. (Notice that the scores of the Dissimilar Group are distinctly skewed.) Calling those subjects who had 61 per cent or over 'high-scorers', and those with 60 per cent or less 'low-scorers', it can be determined that the chi square value is 24·06. This value means that the proportion of 'high-scorers' is significantly greater in the Dissimilar Group at well beyond the ·01 level of probability.

TABLE XIII

Saving Score (per cent)	Less than 20	21–40	41–60	61–80	81–100
Dissimilar Group (N = 30)	0	1	4	11	14
Similar Group (N = 30)	4	10	11	4	1

Problems of interpretation. Before interpreting the above difference between the two groups as being the outcome of retro-active interference, two alternative interpretations must be considered.

1. Might not the difference be due, in some way, to the two groups not being matched for learning ability? This question can be answered by discovering the average number of trials taken by each group to reach the criterion in learning List One. In fact, the Dissimilar Group required an average of 11·63 trials and the Similar Group an average of 11·60. These averages are almost identical so there is no reason to suppose any inter-group difference in learning ability.

2. Might not the time elapsing between the completion of learning List One and the start of relearning List One be greater for the Similar Group than for the Dissimilar? If this were the case and if forgetting were a function of elapsed time, then the inter-group

difference in saving score might have little or nothing to do with retroactive interference. This question can be answered in so far as the timing of the experiment has been properly controlled. The Similar Group required an average of 11·43 trials to learn the interpolated list of eight syllables: each trial involved nine exposures (this includes the list heading) of 3 seconds each: there was an average of 12·43 inter-trial intervals of 12 seconds each. From these facts, it can be calculated that an average of 458 seconds elapsed between learning and relearning. Likewise, from the fact that the Dissimilar Group required an average of 8·83 trials to learn the interpolated list of thirteen adjectives, the elapsed time for this group averaged 489 seconds. Thus the Dissimilar Group had the longer interpolated time interval. This finding illustrates the principle that forgetting is not caused by mere lapse of time but, rather, by the activities which take place during the elapsed time.

Additional results. Any psychological performance has many aspects and, in experiment, some are selected for particular observation. But the opportunity should always be taken to note other aspects even though the conditions of observation do not permit any firm conclusions to be drawn. In the present experiment, two aspects of the performance may be noted in passing and it should be obvious that each may be studied by further experiment.

Effort after meaning. Although the material to be learned is *non sense*, the subject characteristically tries to make sense of it, infuse meaning into it by relating it to what is already familiar to him. If the subject is asked to report on this facet of his activity, he will reveal that: his memorizing and recalling were not achieved altogether passively but by a fairly intricate 'processing' of the material which might be called thinking; those items which were memorized particularly easily were those with most meaning for him; and his investing an item with meaning was, in some cases, responsible for a persistent erroneous recall of this item. (If the subject were to continue to overlearn the list, *automatization* would set in, that is, the correct sequence of items would, more and more, be 'run off' rather than 'thought out'. It is of interest that, in conducting his classical experiments, Ebbinghaus *trained himself* to eliminate effort after meaning from his performance.)

Serial position effect. There is a tendency for items in the middle of the list to be memorized slowest and forgotten soonest. This tendency is yet another manifestation of interference, this time between different items in the same list rather than between one list and another. To study this effect it is necessary to record the response made by the subject on each opportunity for anticipation. Furthermore, since some items are intrinsically easier to memorize than others, it is necessary to obtain results from several subjects, each of whom memorizes a list containing the same items in different orders. (A complete counterbalancing of item and position in, say, a twelve-item list requires the construction of a large number, actually factorial 12, of alternative lists. This number may be reduced by counterbalancing not individual items but blocks of items. Thus, the list may be treated as comprising three four-item blocks and a complete counterbalancing of the positions of these blocks achieved by constructing only six different lists.) This 'block procedure' was carried out by introductory psychology students at Edinburgh University. The twelve items were: PIW – ZUP – WIJ – KEB – GAX – JIQ – BOF – MEC – YUJ – QOS – XAV – FEH. Each of the six derived lists was memorized by seven subjects. For each subject, the ease of memorizing each item was assessed by counting the number of trials on which that item was correctly anticipated and expressing this number as a percentage of the total number of trials required to reach the learning criterion. The combined results for the 42 subjects were that the items of the first, middle, and last thirds of the list were correctly anticipated on an average of, respectively, 69, 39 and 53 per cent of trials. These data reflect the typical bow-shaped serial position curve in which the items most easily memorized are those at the beginning of the list, the items next easiest are those at the end of the list, and the most difficult are those in the middle.

The remarks just made should not be generalized to yield the assumption that the serial position curve obtained with the anticipation method holds for other kinds of remembering performance. When a list of syllables or unrelated words or numbers is read out to a person who is then asked to write down as many of the items as he can in any order he chooses, the best recalled items (which are also the items recalled earliest) are those at the end of the original list, the next best those at the beginning, and the worse those in the middle.

On the other hand, the list of items read out may be sequentially related as in a story, or even only apparently sequentially related as when each item is a short sentence descriptive of a single person, place or event. In recalling such material, it is the *early* items in the list which fare best, the end items next, and the middle items worst. (See Deese and Kaufman 1957. Their materials and methods are readily adaptable to the needs of a successful classroom experiment. This type of experiment leads to considerations of immediate memory and the distinction between short-term storage, where the interval between learning and remembering is a matter of a second or two, and long-term storage. See Broadbent 1958, chap. 9. Broadbent also gives a not-too-difficult overview of one of the most recent and fruitful developments in psychology, namely, the importation of 'information theory'.)*

EXPERIMENT III: EFFECT OF SET ON SPEED OF RECOGNITION

Purpose. To compare the effect of specific and non-specific sets on speed of recognizing.

Material.† For each subject, an array of 36 five-letter anagrams which is divided into six lists containing six anagrams each. The anagrams in three of the lists are categorized, that is, the solution of each anagram in the list is a word which belongs to the same category, e.g. the name of an animal. Three of the lists are uncategorized, the solutions belonging to assorted categories. Movable aperture. Stop-watch or ordinary watch with a good seconds hand.

General remarks. An anagram is a word (the solution word) which is disguised by a disarrangement of the constituent letters. The penetrating of this disguise exhibits characteristics both of problem-solving and of recognizing. The problem-solving aspect is particularly evident when the disguise is difficult to penetrate and the subject engages in sequences of more or less deliberate try-and-check activities, e.g. combining, uncombining and recombining various letter groups. Recognizing is evident in that the disguise is finally penetrated when the subject, in effect, says that this is such-and-such a word. The primary aim of this experiment is to have someone attempt to recognize the solution word under two conditions. (*a*) When the anagram is categorized and the category is known to the subject beforehand. (*b*) When the anagram is uncategorized so that the subject starts off ignorant of the category of the solution word. A secondary aim is to study some of the features of problem-solving activity and, in so doing, demonstrate the interdependence of recognizing, recalling, problem-solving, and of mental processes generally.

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* It is of interest to note the use of nonsense syllable technique here and in chap. 4 on Learning. [Ed.]

† For the anagrams to be used, see p. 23.

'Set' is a concept with which many psychologists have found difficulty. Nevertheless, no one denies that such a concept, in some form or another, is necessary. Briefly, set may be defined as a preparatory adjustment or readiness for a particular kind of activity or experience (for an extended discussion of set, see Johnson 1955, chap. 6). For the purposes of the present experiment, the following characteristics of set should be kept in mind. (*a*) Here set is established by verbally telling the subject the category of the solution word. (*b*) Even with uncategorized material, the experiment still implies set; for the subject treats the material as an anagram, not – as he might do under other circumstances – as a nonsense syllable to be learned nor as a cypher where the letters are to be replaced by digits or by other letters. The difference between the categorized and uncategorized conditions is one of degree only, i.e. of the explicitness of set, not of its presence or absence. Very little, if indeed any, remembering is done with no set at all. (*c*) Non-verbal sets may establish themselves in the course of this, or of any other, experiment. (Example is said to be *better* than precept.) If, for instance, the first few solutions of an uncategorized list were to be names of animals, the subject would be apt to 'look for' more animal names, without any instructions to develop such a set. In the present experiment, the solution words of one category are all proper nouns: this category has been found to be difficult because, apparently, the subjects have set themselves, without instructions, to find only common nouns among the uncategorized anagrams. (A variation of the present experiment is to compare the times taken to recognize categorized and uncategorized anagrams *without* informing the subjects of the condition under which they are, at the moment, working. It is generally found, even so, that categorized responses are quicker. This is recommended as an additional experiment.) (*d*) When a subject has recently recognized a solution word, he will more readily recognize this same word on a second presentation of its anagram (even if the anagram be different). This further exemplifies the influence of set; and the reader is invited to test this generalization by experiment.

The last-mentioned fact complicates the present experiment to the extent that the same anagram cannot be presented more than once to the same subject. A further complication stems from the fact that anagrams vary intrinsically in difficulty even when allowance is made for ability, educational and other background, shifting set, and so on. Some solution words are less familiar: some anagrams involve greater disarrangement of letters, e.g. anagram 12354 is easier than 42513 when the solution is 12345 (this has been verified by Hunter 1959 – this paper should be consulted for yet another classroom experiment involving anagrams and for an extended account of anagram solving activity). In order to overcome these two complications, the material for this experiment has been constructed as follows.

There is a total of 54 different anagrams, six from each of nine categories which may be symbolized by the letters a to i. These 54 anagrams are arranged into three arrays of 36 anagrams each; and each array contains six lists numbered from 1 to 6. Lists 1, 3 and 5 are categorized while lists 2, 4 and 6 are uncategorized. In Array One, the three categorized lists contain all six words from categories a, b and c, respectively; each uncategorized list is 'mixed' and contains one solution-word from each of the categories d, e, f, g, h, and i. In Array Two, the three categorized lists are d, e and f, while each uncategorized list contains one anagram from each of a, b, c, g, h and i. In Array Three, the categorized lists are g, h and i; the uncategorized are made up from a, b, c, d, e and f. Taking these three arrays as a whole, each of the 54 anagrams occurs twice, once as categorized and once as uncategorized. It should be noted that each anagram usually permits of one, and only one, solution.

Experimental procedure. The following is recommended as one of several satisfactory procedures. Persons work in pairs, one acting as experimenter and one as subject. Each experimenter is provided with an array of 36 anagrams, a movable aperture and a watch. The subjects are told that they will try to solve six lists of anagrams, each list containing six different anagrams: three of the lists will be categorized, three uncategorized. Each individual anagram will be exposed for 30 seconds but the subject should call out the solution as soon as he is sure that he has recognized it.

The experimenter's task will be as follows. He covers the array, places it in front of his subject, exposes the heading of the first list and announces whether the list is categorized (when he reads out the category, e.g. animal) or not. Then, after alerting his subject, he exposes the first anagram of the first list, simultaneously starts his watch, and keeps the anagram exposed for a maximum of 30 seconds. When the subject calls out a solution, the experimenter notes the time. If the solution is given before 30 seconds is up, the experimenter records the recognition-time and also the word stated as recognized and, this done, exposes the next anagram in the list: if the anagram is solved, there is no need to wait until 30 seconds is up before proceeding to the next item. If the anagram is not solved in 30 seconds, the experimenter records 'fail' and moves on to the next anagram. This procedure is continued to the end of the list and repeated with the five remaining lists.

Difficulties of timing. The measurement of each recognition-time and of each 30-second limit must be as accurate as possible. But it would be overzealous to record to anything smaller than the nearest second. An accuracy of anything smaller is likely to be impossible under the circumstances; as is an accuracy of less than about two seconds in the case of 'instantaneous' recognitions. Another timing difficulty arises when the subject 'jumps the gun' by calling out a wrong word and then withdrawing it. As a precaution against the confusion of such a circumstance, the experimenter should keep his eye on the second hand of his watch, observe the time at which the word is called out, and let the watch run on for some seconds in case the subject should offer another word. The time elapsing between one exposure and the next need not be kept rigid: the experiment is best conducted at a leisurely pace so that neither experimenter nor subject feels rushed or flustered.

In this way, each subject attempts three lists of each type. Where the number of persons in the class permits, the further refinement should be introduced of alloting one subject to each of six slightly different tasks. One subject takes the lists of Array One in the order 1, 2, 3, 4, 5 and 6; a second subject takes the lists of Array One in the reverse order, beginning with 6 and ending with 1 so that he starts with an uncategorized list. A third subject takes Array Two in the forward order, a fourth Array Two in reverse order. A fifth takes Array Three in forward order and a sixth in reverse order.

Report by the subject. When the subject has finished, he should be asked to comment on his performance. Did he, for example, feel that the specification of the solution word's class helped him and, if so, in what ways? Did he ever recall any words fulfilling the specification before going through the specified lists? Was he aware of using any definite procedures in trying to achieve recognition? Was he aware of any particular obstacles in the way of recognition? The subject's answers to these and other questions may reveal much about his performance which could not be discovered by a consideration of his time scores alone.

Treatment of results. To begin with, the results of each subject should be considered separately. The overall performance is represented by 36 time scores. Some of these give recognition-time directly; others, the 'fails', signify that recognition-time was 30 seconds *plus* some further unspecified time – it cannot be known whether recognition would have been achieved after 31 seconds or whether a whole day might not have been required. Eighteen scores are made on categorized, eighteen on uncategorized material, and the critical comparison lies between them.

Use of failures for comparison. The two arrays of eighteen scores cannot be compared in terms of their means because (1) each distribution of scores will be skewed (unevenly distributed about the mean) which invalidates the usual comparison procedure, and (2) 'fails' cannot be assigned a precise time value. Comparison can, however, be made in terms of the number of 'fails' among the two distributions. The prediction is that there will be more 'fails' on the uncategorized anagrams. Note that a 'fail' is based on an arbitrary time limit of 30 seconds and that a more satisfactory assessment is obtained by taking as the 'time limit' the median ('midmost') recognition-time of all 36 anagrams. This gives an approximately equal number of 'fails' and 'passes' over the total array of anagrams and the question is: how many of these fails belong to the categorized anagrams and how many to the uncategorized ones? The use of the *post hoc* time limit shows most clearly any differences in fail rate between the two conditions.

An alternative (and basically similar) comparison is carried out in terms of the median recognition-times under the two conditions. The ranking begins by taking the 'fails' as the largest time scores (30+ seconds) and then times of 30, 29, 28 seconds and so on until the tenth and ninth largest scores are reached: the median of the 18 scores is, nearly enough, the average of these two scores. If there are nine or more 'fails', the median of the array is 30+ seconds. The prediction is that the median recognition-time of the 18 categorized anagrams will be the smaller. As here stated, this prediction is rather gross; but in fact, it is possible to calculate the likelihood that the two medians differ to an extent which is greater than would be obtained by chance alone. (An account of this calculation is given by Siegel, 1956.)

Problems of interpretation. Suppose the subject's failure rates or recognition-times confirm the above predictions. Can it be concluded that this demonstrates the facilitating effect of set on speed of recognition? The conclusion is certainly suggested but it does not automatically follow. There are two further factors which might have contributed to the outcome: without conducting subsidiary experiments, we cannot be certain that they operate; but we cannot assume their absence and, at the least, must be aware of their potential effects.

1. *Sampling error as vitiating factor.* Some anagrams are intrinsically more difficult to unmask than others. So it might happen that one population of anagrams (either categorized or uncategorized) has been chosen which is, quite apart from any effects of set, more difficult or easy than the other. This sampling error can be eliminated by presenting each anagram twice, once under each condition, so that the two populations are identical. But if only one subject is being studied, this double presentation is impracticable because it introduces the further factor of familiarity with the anagram and its solution. The extent of sampling error can, when only one subject is being used, be reduced by increasing the number of anagrams to be taken under each condition; and the use of the relatively large

number of 36 anagrams in the present experiment is an attempt to do just this. But though a population of 36 anagrams reduces the sampling error considerably, it does not altogether eliminate the possibility of error. The only way to make sure is to present the same anagrams to different subjects but under different conditions for each subject. With the material given, this counterbalancing of anagrams and conditions is achieved by having one subject take Array One, another Array Two, and a third Array Three. If all three subjects have a longer median recognition-time under the uncategorized condition, we can be fairly certain that their collective results are not due to sampling error.

2. *Practice effect as vitiating factor*. It may be that as the subject proceeds from anagram to anagram he becomes more proficient at his task and progressively quicker in achieving recognition. Alternatively, he may become fatigued, bored or upset and so become progressively less proficient. (Such progressive changes as may exist are not necessarily revealed by studying the time scores because of the confounding effects of variations in the intrinsic difficulty of the anagrams.) Because of these possible practice effects, it is desirable not only to intersperse the two kinds of list but also to have some subjects start with one and others with the other. This counterbalancing of conditions with the practice factor is achieved by having one subject start an array of anagrams at list 1 and another at list 6.

It will be realized that any counterbalancing procedure does not prevent 'extraneous' factors from operating. What it does is to nullify the effects of these factors on the final outcome of the experiment.

The results taken as a whole. When the results of each subject have been calculated, the results of all the subjects should be collected. If the tasks of the different subjects counter-balance each other in the manner described above, it can be concluded with a high degree of certainty that specification of the solution word's category has facilitated speed of recognition. From this and other similar experiments, the general principle emerges that: *the more specific the set, the easier, in terms of speed, the recognition.*

Specimen results. The experiment, exactly as described above, was performed with a total of sixty student subjects Every single subject had both a higher median recognition-time and a larger number of fails (at 30-second limit) on the 18 uncategorized than on the 18 categorized anagrams. Over all arrays and conditions, the median solving time was 20 seconds for uncategorized and 4 seconds for categorized items. The average number of fails (at 30-second time limit) was 7·12 for the uncategorized and 1·97 for the categorized items.

Retrospective comments of subjects. These reveal supplementary findings, some of which are as follows.

Suddenness of recognition. Whether recognizing is achieved soon or late, it is achieved with characteristic suddenness. To the subject, there is an abrupt transition from the confusing jumble of the anagram to the meaningful familiarity of the solution word.

Perceptual change. Nature of recognizing. Once the transition to recognition has taken place, the subject's perceiving of the anagram is altered – for a time at least. It 'looks' different and is so suggestive of the solution word that it is often difficult for the subject to understand why he took so long to achieve recognition. Recognition is perception characteristically altered by past experience in that what is recognized (1) produces a feeling of familiarity and (2) is perceived as having special attributes not directly given by the objective situation. For example, to a motorist, the green light of a traffic signal 'looks' permissive and the red prohibitive; a 'hard looking' person may, on closer acquaintance, be perceived as 'noble' or even 'kindly'.

Our everyday perceiving is riddled through and through with the effects of past learning. The present experiment affords modest illustration of this mighty fact in that, after recognition, the anagram's disguise often seems meagre and transparent. Those experimenters who try to solve the anagrams along with their subjects, and achieve recognition before their subjects, remark on their bewilderment at the subject's inability to see what seems obvious. Similar bewilderment is experienced by most people from time to time, especially in their dealings with young children. Realization that the 'obvious' usually becomes so only as a consequence of learning should make us more tolerant of the 'stupidity' of those who have not shared the relevant aspects of our own past experiences.

Subjective aspects of set. When set is induced by verbal instruction, subjects are often aware of a subjective change even though they may find difficulty in reporting its exact nature. (Note that sets may also be induced by ways which result in no awareness, on the subject's part, of set.) For some subjects, the altered awareness is vivid, e.g. given the category of 'fruit', they may 'see themselves' in a fruit shop surrounded by an array of fruit and vegetables: in some cases, the imaged shop relates to an actually known shop and is experienced in lively detail. For others, the changed awareness may be vague, elusive and quite devoid of imaging: their experience can only be described as one of readiness, of being appropriately oriented towards the task to be presented. For yet other subjects, it may happen that set is experienced as more than either an imaged situation or an orientation: they may 'spontaneously' recall words fulfilling the requirements of the given category. In effect, they may say 'Fruit? Well, there are apples, oranges, bananas, lemons, plums . . .'

To achieve recognition, there must be an interaction of past experience (or rather the retained effects of such experience) and present perceiving. Sometimes subjects attend mainly to the perceived anagram and manipulate its letters until, under the guidance of set, a rearrangement 'fits' an appropriate and already familiar word: the anagram gives out, as it were *exudes*, the solution word. Sometimes subjects attend rather to the familiar words: they recall words, again under the guidance of set, until one 'fits' the anagram: the word, as it were, *absorbs* the anagram. The contrast between these two routes may be sharpened by analogy. Consider a door with several locks and a person who wants to open the door and has a bunch of keys for the purpose. He may select one lock and search for a key to fit it or, alternatively, he may select one key and search for a lock into which it fits.

Problem solving activity. It has been mentioned that the unmasking of an anagram involves not only recognizing but also problem solving of a simple kind. The following phenomena are characteristic not only of anagram solving but of problem solving in general (see also chap. 6).

1. Solution occurs, broadly speaking, in one of two ways. (*a*) 'Spontaneously' or 'implicitly' and without any awareness of effort expended; the solution is 'just there' all of a sudden. (*b*) 'Explicitly' after a more or less prolonged and consciously explicit sequence of try-and-check activities. In the present experiment, an awareness of the distinction between 'instantaneous' and 'explicit' solutions influences the attitude of quite a few subjects towards their

task. They approach each anagram with passive expectancy and, if the solution does not 'come', experience mild annoyance and despair prior to 'getting down to the job' of 'trying things out'.

2. 'Explicit' solutions characteristically involve chains of steps in which various procedures are tried out, examined and rejected as unsatisfactory. For the most part, these procedures involve the regrouping of letters into pairs or triplets and the distribution of consonants around vowels. The alert subject may notice that he consistently tends to try out certain groupings rather than others. He may, for example, pair 't' with 'h' whenever these letters occur in the same anagram; or where only one vowel is present, place it in the centre of the still-to-be-discovered word; or where there are two vowels, insert a consonant between them. It will be found that these preferred groupings are not a matter of chance but reflect fairly closely the letter groups which are, in point of fact, most frequent in English words. In anagram solving, as in problem solving generally, past learning affects the nature of the steps taken towards solution.

The subject may occasionally observe himself carrying out a try-and-check sequence of a different kind. He provisionally decides that one particular letter or letter combination will occupy a certain position in the solution words (the beginning, say, or the end) and then tries to recall a word which fits this provisional skeleton, e.g. what short word begins with a 'v', or ends with a 't', or has a double 'l'?

3. Having achieved a certain letter grouping or a provisional skeleton of the sought-for word, the subject may find himself an unwilling prisoner. He tries, for example, to break up the letter combination 'ale' but finds it peculiarly difficult to do so; and until he splits up this triplet, he cannot recognize the word 'ankle'. This difficulty is of a kind which often arises in problem solving and is known as a difficulty in *restructuring* (in everyday life we speak of 'getting in a rut'). As a result of habit or even of genuinely novel thinking, the subject 'sees' certain components of his problem in one particular way and cannot 'look at' these components afresh and in some different relationship to each other: the parts become, as it were, stuck together. This fixedness exemplifies yet another variety of set: one which is, in most cases, of the subject's own unwitting creation and one which operates to impede rather than facilitate successful completion of the task.

4. Anagrams are usually encountered in crossword puzzles where the clue does not explicitly state that an anagram is involved. Here, a large part of the difficulty is to discover that an anagram is called for, to recognize the problem as being of the anagram type. Bartlett (1932) noted that a strange word, picture or phrase was reacted to with an 'effort after meaning' and that this meaning was achieved, if at all, by relating the new and unfamiliar to the old and familiar: this done, the person can 'deal' with the new situation on the basis of past experience. The same is true of a person's initial reaction to a strange problem: he tries to relate it to some kind of problem with which he is familiar. During his lifetime, he has met many kinds of problem and learned the procedures (not necessarily the best procedures) for solving them. So if he can recognize a new problem as being an old problem in disguise, he knows what procedures to use in solving it – in the sense that he is set to carry through one or more sequences of potentially relevant activities. In fine, a person may recognize events much more complex than a word or a nonsense syllable; he may recognize a problem category. In many problem situations, this recognition process occurs so rapidly that it goes unnoticed. In the crossword, the activity of recognizing a problem as being of a certain type (an anagram) is more obvious. In many everyday situations, this recognizing aspect of problem solving is more evident still. Young school children who can add, multiply, subtract and divide often have difficulty in knowing which procedure is appropriate to a textbook problem in arithmetic. University students of statistics who know their statistical procedures well are often perplexed, when faced with a new set of data, to know which procedure to employ. Administrators, faced with a new problem, characteristically search for a precedent. Such examples show that, when a problem arises, the person's initial activity (and sometimes his greatest difficulty) is to recognize the category of the problem.

REFERENCES

BARTLETT, F. C. (1932). *Remembering*. Cambridge University Press.

BOUSFIELD, W. A. (1953). 'The occurrence of clustering in the recall of randomly arranged associates'. *J. gen. Psychol.*, **49**, 229–40.

BROADBENT, D. E. (1958). *Perception and Communication*. London: Pergamon.

DEESE, J., and KAUFMAN, R. A. (1957). 'Serial effects in recall of unorganized and sequentially organized verbal material'. *J. exp. Psychol.*, **54**, 180–7.

EBBINGHAUS, H. (1885). *Über das Gedächtnis*, translated 1933 as *Memory*. New York: Teachers' College, Columbia University.

HEBB, D. O. (1958). *A Textbook of Psychology*. London: Saunders.

HILGARD, E. R. (1951). 'Methods and procedures in the study of learning', in *Handbook of Experimental Psychology*, ed. Stevens, S. S. London: Chapman & Hall.

HUNTER, I. M. L. (1957). *Memory*. Harmondsworth: Penguin.

HUNTER, I. M. L. (1959). 'The solving of five-letter anagram problems'. *Brit. J. Psychol.*, **50**, 193–206.

JOHNSON, D. M. (1955). *The Psychology of Thought and Judgment*. New York: Harper.

KOFFKA, K. (1935). *Principles of Gestalt Psychology*. New York: Harcourt Brace.

OSGOOD, C. E. (1953). *Method and Theory in Experimental Psychology*. New York: Oxford University Press.

SIEGEL, S. (1956). *Nonparametric Statistics*. New York and London: McGraw-Hill.

Chapter 4

LEARNING

A. J. WATSON

Cambridge University

The predominant line of thought in the study and explanation of learning has been what may be called, in a broad sense, the doctrine of Associationism. Now, to say that in learning two or more things are 'associated', where previously there was no association, comes very close to tautology; or, better, to part of an explanation of what we mean by 'learning'. If I am interested in how it is possible for a man to modify his behaviour in the way we call learning, I make no great advance in saying that the learning was due to 'association'. It is this association which, usually, is the fact to be explained. Nevertheless, there are two limitations to this analysis. First, in the course of its long development in the hands of philosophers, for instance in such works as those of the British Empiricists, the concept has tended more and more to acquire a quasi-mechanical connotation. It has ceased to be purely descriptive of certain aspects of the learning process. This tendency has been carried further in empirical psychology. Thus, we find the use of such terms as associative 'bonds', 'links', and 'connections'. Sometimes we find discussion of the formation of 'associative pathways', and the like. These are, clearly, all attempts to pass beyond the description of the behavioural characteristics of learning to the kind of mechanisms which control such behaviour. An attempt is being made to explain how learned modifications can occur in terms of some hypothetical machinery. But very little progress has been made merely by the use of such terms in a way which has only the appearance of reference to controlling mechanisms. For, after all, what do we know about 'bonds', 'connections' and so on? Do we know what we mean when we use these terms in a sense other than that in which 'A connection has been established in Timothy associating the sight of fire with pain' means something like 'Timothy has learned that fire hurts'?

In order to make the use of such words informative, at least one of two things seems necessary. We might have some idea of what a 'connection', 'association', etc. is in physiological terms, and have some reason for thinking that the physiological change or process concerned stood in some causal relation to the modification of behaviour which we call learning. Alternatively, we might define the notion of associative connection in other ways, without reference to physiological embodiment, and specify the kinds of things between which such connections are made, the conditions which are required for their formation, and so on. These two approaches are not to be regarded as mutually exclusive. For we might begin with some physiological notion of association and arrive, by a study of the conditions under which learning occurs, and fails to occur, at a better understanding of the role of these physiological processes in the control of behaviour and particularly in learning. Or, by the study of the kinds of events which are in fact 'associated' in learning, and of the conditions necessary for this to occur, we may arrive at a more precise idea of the function of such connections, and we may then search for the physiological basis of them.

Now, one of the early examples which combined both these approaches to learning, together with an experimental analysis of the conditions required for the formation of such connections, is the theory of conditioned reflexes developed principally by Pavlov. Conditioned reflexes, in Pavlov's sense, consist in the establishment of 'connections' between two areas of the brain, namely those excited by the reception of the conditioned and the unconditioned stimuli. The area concerned with the reception of the unconditioned stimulus is supposed already to possess further connections with those motor areas, the excitation of which will elicit the response to be conditioned. These connections are referred to in physiological terms, and presumably the process of connecting the areas for the conditioned and unconditioned stimuli depends upon rather vaguely specified physiological changes. The specification of the conditions required for the formation of such 'connections' is simple. The two stimuli, and hence the excitation of the two reception areas, must regularly occur in fairly close temporal proximity (except in the special case of delayed conditioned reflexes which will not be discussed now); the conditioned stimulus should not follow the unconditioned stimulus; and the brain must be in a generally 'excitable' condition. The first two of the requirements are clear. But the last, although essential from Pavlov's point of view, is vague and imprecise, and leads to difficulties. Before discussing these, a method for demonstration of the basic phenomena will be described.

DEMONSTRATION I. CONDITIONED REFLEX

Method. Generally speaking, it is more difficult to produce unequivocal conditioned reflexes in people than it is in animals. The reasons for this are of some interest, but discussion of them will be postponed. In order to condition some human response, we have first to select the 'reflex' to be used. The degree of conditioning obtained in people tends to vary with the kind of response involved. Thus, motor responses such as the knee jerk, limb movements in response to electric shocks and eye-blink reactions to light and sound have been utilized; so also have salivation, pupillary responses to light and dark and the psychogalvanic reactions. None of these gives perfect results, but the psychogalvanic response and eye-blink may be conditioned with reasonable reliability in the majority of subjects without very lengthy training procedures. The 'reflex' suggested here, however, is the response of the hand to electric shock. This response may be conditioned with fair consistency in many people. And the choice has the advantages of requiring little equipment and of presenting a problem, which arises in the interpretation of the results of conditioning experiments, rather more clearly than do the other procedures customarily employed when people, rather than animals, serve as subjects.

We require first, then, a means of administering a suitable electric shock to the hand of the subject, who may be a volunteer from the class. This may be achieved by using an adjustable induction coil which delivers the current through two electrodes, one in contact with the palm of the subject's hand and the other touching one or more fingers. (Control of electrical circuits is conveniently by foot under the table in view only of the class.) Although the intensity of the shock delivered by this means will be far from constant from trial to trial, and from person to person, due to variations of skin resistance, etc., if the shock is fairly strong such variations within and between experimental series will not be important for demonstration purposes.* The induction coil may be adjusted to give a suitable output at the beginning of each experiment. This value should be such that the subject can endure a fairly long series of shocks and yet a rapid flexion and withdrawal movement of the subject's hand should be elicited. There is, of course, a wide range of combinations of voltage and current which will produce this effect, and the choice from these will be determined by the apparatus available. Conditions of safety require either that the voltage used be fairly low, or that a limit be imposed upon the possible output current from the coil.†

The subject is seated and one of his hands rests horizontally upon some support. One of the electrodes is placed upon the support so that, when the shock is administered and the hand moves, the subject escapes from shock; or both the electrodes are strapped or tied to the hand so that the response does not terminate the shock. Many have found, in a situation such as this, that the latter procedure is the more efficient. This is in contrast with the general finding in experiments using animal subjects, and this point will be discussed further below. We have, then, a situation in which an unconditioned stimulus, shock to the hand, elicits a reflex response which does not eliminate the stimulus. Almost any easily discriminable cue may be used as the conditioned stimulus. It is, however, necessary that this stimulus should not also, without training, elicit the hand reaction; and this point should be tested with whatever stimulus is chosen, before the conditioning procedure is begun, by presenting it several times in the absence of the shock. It will be assumed here that a buzzer will provide the conditioned stimulus. (The 'buzz' of the induction coil should be muted.) It occasionally happens that subjects will respond to almost any stimulus in a manner similar to their reaction to shock. This is one of the disadvantages which arise from the use of this method with people, for, even in the absence of preliminary instruction, the electrodes have a sinister aspect which will rarely escape the subject. Any sudden and pronounced stimulus will tend to provoke, both in people and in animals, some kind of startle reaction. In the present situation, the subject being more or less aware of the purpose of the experiment, this is likely to include a kind of withdrawal response which may, in some cases, be difficult to distinguish from the subsequent conditioned response. The best chance of eliminating this effect lies in choosing as the conditioned stimulus a signal which, though distinguishable without difficulty, is fairly restricted and not too vigorous. Thus, the note of the buzzer should not be louder than necessary; if light is to be used as the conditioned stimulus, a small patch of light on a screen should be presented, and so on. Given these precautions, it is unlikely that the conditioned stimulus will initially evoke the response which we wish to condition to it. But if, in a given subject, the response is obtained in these preliminary tests using the conditioned stimulus alone, we have no choice but to use a different stimulus or a different subject. For, clearly, we cannot demonstrate the acquisition of a 'connection' which seems already to be present.

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* Another satisfactory method is to strap a piece of flexible copper, say 15 × 4 inches, to which is soldered one electrode, completely round one upper arm; the *other* hand rests on an 8 × 10 inches piece of copper, nailed to a soft wood laboratory table with the other electrode soldered on to the copper. This has the advantage that the moving arm is free and completely visible. It is important that the bandage be not too tight, and that the 'strength' of the shock be carefully adjusted.

† A car battery (6 v.) has been satisfactorily used.

A further difficulty arises concerning the instructions which should be given to the subject. It is clear, at least in the case of people, that their initial reactions to the situation will vary considerably in terms of what they believe is expected of them. Thus, one person may think he should be quite passive, another may think that any movement of his hand will disrupt the experimental procedure, a third may approach the task rather as a test of his endurance, and so on. One tends to feel this difficulty less acutely when animal subjects are used. But, although this particular aspect of variation between subjects may be less troublesome when animals are studied, many sources of variation remain. In the present instance the variability may be reduced to some extent by the use of more or less standardized instructions. The subject may be told that shocks will be administered to his hand at irregular intervals, that his hand may move slightly, 'automatically', as a consequence of these, that he should not attempt to facilitate or inhibit this response which the experiment is designed to investigate, that some other stimuli may be presented in the course of the series of shocks, and so on. It is, of course, essential that these instructions are not carried to the point at which the subject's response may itself be regarded as the execution of the instructions given to him. If, for instance, we were to inform him explicitly that we were concerned to investigate how soon he would learn to associate the buzzer with the shock, the point of the experiment would be lost. Although it is desirable that the subject should receive preliminary instructions to the effect that shocks are to be expected and so on, and although these may serve to reduce the variability between subjects, it would be idle to pretend that this variability will thus be eliminated, or that all subjects will interpret the instructions, which in the nature of the case cannot be too explicit, in the same way. But it is the best that can be done and we must rely upon careful and detailed observations of the subject's behaviour, and upon his replies to questions after the experimental series, in order to determine how far any two subjects have exhibited the same kind of behaviour for the same reasons.

The following instructions have been used with considerable success. *This is an experiment in learning. It is not a test of nerve or of endurance or of ability to be deceived. You are asked to behave in an entirely natural way, not moving from the chair but otherwise acting as you might imagine an animal would do in the same situation. Please do not make up your mind that you will, or will not, behave in a certain way. We are trying to find out what normal human beings do, not how much they can stand or whether we can deceive them.* It may if desirable be explained to subjects beforehand that these are the instructions ordinarily given to subjects who know nothing of psychology. Instructions may of course be repeated if desired by the subject. Successful experiments have been conducted with the subject himself reading the instructions.

After the preparation of the apparatus and subject, and after the initial tests to determine the neutrality of the conditioned stimulus, the conditioning proceeds. The temporal relations of the conditioned and unconditioned stimuli are of great importance. It will probably be found that the most efficient procedure is that in which the conditioned stimulus precedes the unconditioned by about ½–1 second. Conditioning in a reasonable number of presentations has, however, been obtained when the two stimuli are presented simultaneously. The choice of interval here involves not only a question of efficiency of conditioning. The *kind* of conditioned response obtained tends to vary with the interval. For this demonstration, great precision of timing is not required and it should be possible for the experimenter to maintain a sufficiently constant interval by manual operation of the controls of the stimuli. The intervals between successive presentations of shock and buzzer should be irregular. For, if the combination recurs regularly at, say, 20-second intervals, it will be found either that no conditioned reflex is established, or that trace conditioning is obtained. That is, subsequent tests will reveal that the hand response has been conditioned at least partially to the time interval between presentations, and the buzzer, when presented alone, will be followed by the response only if it coincides roughly with the end of the inter-trial interval established during training. It is suggested that the time intervals between successive presentations of the buzzer-and-shock combination should *vary randomly in the range 10–25 seconds, having a mean value somewhere about 15 seconds*. Lastly, it has been found in animals in this kind of situation that conditioning is usually achieved most rapidly if the conditioned stimulus ends as the unconditioned stimulus begins. Although the supremacy of this arrangement is likely to be less pronounced in the case of human subjects, a satisfactory procedure would be to sound the buzzer for about 1 second, switch off the buzzer and switch on the shock simultaneously, and finally switch off the shock after a further 1 second. (The two *time intervals*, i.e. (1) between buzzer and shock, and (2) between *combinations* of these, must be distinguished.)

A series of conditioning trials should be presented in these time relations. It is *advisable* first to prepare a programme of the random inter-trial intervals and of those occasions upon which the buzzer will be presented alone to determine whether conditioning has yet taken place. It should be arranged that these test trials are not too frequent, for otherwise an appreciable extinction effect will arise which may prevent the formation of any pronounced overt conditioned reflex; and, also, they must not be presented regularly, for in this case the subject may well learn that on every nth trial shock is omitted. In this demonstration these conditions will be met if the buzzer is presented alone, on average, every ten trials, but at random points in the sequence. It is difficult to specify what degree of response to the buzzer alone should be taken as evidence of the formation of a conditioned response. The character of the movement will vary with each subject, the characteristics of the shocks, the placing of the electrodes, and so on. Any criterion is somewhat arbitrary and is best determined in each particular situation. A response which bears a reasonable descriptive similarity to that elicited by the shock itself, and which is evoked by the buzzer alone with some reliability, may be taken as sufficient. When such a response has been obtained to the buzzer alone twice the frequency of the presentation of such test trials may be increased, say, to three in the following ten trials. If these further test trials also evoke the response, this part of the experiment is completed. If, however, this criterion is not met, we must repeat the original conditioning procedure, followed by more special tests, and so on. There will be great variability in the speed with which conditioning will develop from session to session; but, usually, we may expect this procedure to produce reliable conditioning in well under one hundred trials, and *first* conditioning in about thirty.

Discussion. Before proceeding to examine further effects which may be demonstrated in a subject thus conditioned, several points which arise from this simple demonstration deserve mention. The first of these concerns controls necessary to establish that an association has in fact been established between the conditioned stimulus and the response. The requirement that the conditioned stimulus should not originally evoke the response has already been mentioned. Attempts have sometimes been made to evade this requirement by comparing the degree of response to the unconditioned stimulus before and after conditioning trials. If the latter is greater than the former a conditioned 'connection' is presumed to have been established, or at least strengthened. This argument is rather unsatisfactory. For it rests upon the assumption that a stimulus cannot evoke a given response more vigorously, as a consequence of the repeated production of this response by some other stimulus, by any means other than by the strengthening of a conditioned connection. The plausibility of this assumption varies with the kind of experimental situation employed; but usually there is no direct evidence supporting it. Suppose, in this demonstration, it had been found in the initial test trials that the buzzer alone showed some tendency to evoke a response which included a component similar to the conditioned response. Now the series of shock trials may be expected to place the subject under some stress and this might, in turn, make him more prone to respond to *any* stimulus. It is also likely to make the hand movement particularly 'accessible' in this sense. As it were, the response might become like a gun with a particularly light trigger and any slight disturbance might release it. There is evidence both from common observation and from more experimental analyses of behaviour that this kind of effect sometimes occurs. In the present case, we might then have obtained an enhanced response to the buzzer alone after a series of conditioning trials. But this would be due not to the formation of any 'buzzer-shock connection'; it would be attributable rather to a more general 'toning up' of the control system. This objection may be extended further to the case where the initial trials showed no tendency of the conditioned stimulus to elicit the response to be studied. For we have no evidence that the effect of the conditioning trials has not been only to make the response more 'accessible' in this way. It might be that no 'connection', either in a functional or in a physiological sense, had been established.

This distinction between two kinds of way in which the efficacy of a stimulus, in the production of a given response, may be enhanced is to be found in the original theory of conditioned reflexes and in most subsequent hypotheses developed from it. Pavlov found it necessary to suppose that the condition of the cortex might vary between a comparatively inhibitory and a comparatively excitatory state. In the first condition the activity of all reflexes, both conditioned and unconditioned, will be depressed; while in the second condition they will be more easily excited. This is one of the more vague and unsatisfactory aspects of the theory, but something of the kind is necessary if any account is to be given of changes in behaviour with variations of motivation, of fluctuations in the magnitude of reflex responses with changing intensity of stimulation, of disinhibition, and so on. It seems likely that almost any theoretical account would contain something tantamount to a distinction between the formation of associations and more general facilitatory effects; and so, if we wish to regard this demonstration of conditioning as an example of the formation of an 'associative connection', we require some control condition which gives us grounds for believing that the series of buzzer-shock presentations had more than such a facilitatory effect. There are at least two ways in which this may be accomplished. We may, at the outset of the experiment, present sometimes a light alone and sometimes a buzzer alone. Assuming that neither of these is found to evoke the reaction to be conditioned, one is selected for the conditioning procedure. In addition to the test trials, in which this stimulus (say, the buzzer) is presented alone, an equal number of test trials in which the light alone is presented are included. If it is found that the buzzer, but not the light, elicits the response, we have some grounds for thinking that the effect of the buzzer depends upon an association, rather than upon a more general facilitatory process. The procedure should be repeated on another subject with the roles of the light and buzzer reversed. The second method of control also calls for the services of more than one subject. Some subjects are required to undergo the conditioning procedure outlined above; the others are given the same number of buzzer and shock presentations, but the buzzer is not sounded in combination with the shock. If the buzzer evokes the conditioned response only in those subjects who have experienced it conjointly with the shock, we may conclude that an associative connection has been established. Our confidence in this conclusion will, of course, depend upon the number of subjects employed. If only two subjects are used, it might well be that an apparent conditioned response in the one subject was due to a general facilitative effect of shock, which, for some reason, failed to occur in the 'control' subject. But if a consistent difference between the two treatments were found when a number of subjects were employed in each condition, greater weight will be attached to the conclusion.

The situation suggested for this demonstration was one in which the subject could not escape the shock. If such escape is possible – e.g. not both the electrodes are attached to the hand – and the conditioned stimulus precedes the unconditioned, the subject may quickly learn to avoid the shock altogether by a withdrawal movement. In experiments in which animals serve as subjects, this is much the most efficient way of producing a 'conditioned response' and, indeed, is one of the most efficient training procedures in certain situations. This is not so consistently found where human subjects are used. The importance of the instructions given to the subject has already been stressed, and it is clear that, in the case where avoidance is possible, this point is of especial importance. For any normal subject will soon learn that the buzzer precedes the shock and that he can avoid the shock by lifting his hand as soon as he hears the buzzer. What is he to do? If he thinks that he is free to move his hand as he wishes, or that the point of the experiment is to investigate his ability to associate buzzer and shock, he will soon learn to lift his hand to the sound of the buzzer, and will probably continue to do so indefinitely. Is this a conditioned response? Does the fact that it is purely 'voluntary' affect the issue? If, on the other hand, the subject supposes that nothing so simple as this can have been the experimenter's intention, he may resist all movements for as long as his endurance lasts. It is extremely difficult to frame instructions to the subject which will not tend to commit him to one course or the other; and vaguely or ambiguously phrased instructions are likely to be interpreted differently by different people. One possible wording has been suggested earlier in this chapter.

Considerations such as these lead one seriously to question the identity of ordinary learning procedures and conditioning of the kind described above. For the rapidity of avoidance learning, if it is permitted, merely emphasizes what is in any case obvious, that in this demonstration the subject must have learned or guessed that it was very probable that the buzzer would be followed by the shock long before the buzzer alone reliably produced the hand response.

A number of points arise here. The difference between a voluntary response to the conditioned stimulus in an avoidance situation and the more automatic response in this demonstration lies not only in the subject's feeling of volition, nor in the rapidity with which learning manifests itself. It is found that the nature of the response itself differs in the two situations. Unaided observation of a relatively gross response of the kind exhibited here may not clearly reveal the difference. But in other situations, where a more accurate measure of the time-course and character of the response may be recorded, or where the response is not limited to a comparatively small part of the body, the difference is clear. Generally speaking, in an avoidance situation the movement is a delicately controlled response which may differ very greatly from that produced by the unconditioned stimulus. After sufficient training it seems to obey a kind of rule of least effort; that is, it is the minimum response which will consistently avoid the punishment. In an experiment where avoidance and escape are not possible, however, the conditioned response approaches more to a replica of the reaction to the unconditioned stimulus. But even here the resemblance is limited. The conditioned response, even after a very extensive series of conditioning trials, is usually no more than a kind of anticipatory reaction of the same general class as the unconditioned response, reduced in magnitude and with a different time-course. There is, however, one important similarity between the types of response acquired in the two kinds of situation. For, at least in human subjects, it is found in both cases that the response does not appear until after the appropriate reaction time. (It is an easy matter to put an electric timing clock in the circuit, if available.) The reaction time for the unconditioned response to shock is very much shorter than this. In this sense, the conditioned response is 'voluntary' whatever the possibilities of avoidance or escape.

Thus, although this demonstration may be regarded as reflecting the development of an 'associative connection' in some sense or other, it can scarcely be claimed either that this 'connection' is a simple matter of linking one sensory reception area in the brain to another, or that whatever associative processes it does reflect are typical of learning in other kinds of situation. With these limitations in mind, the results of such conditioning experiments may appear something of a curiosity, and also pose a problem. If 'ordinary' learning is so much quicker, reliable and does not necessarily involve any response similar to that elicited by the unconditioned stimulus, what processes are responsible for conditioning of the kind shown in this demonstration? Before examining these questions further, the exhibition of some additional effects associated with this type of experiment will be outlined.

EXTINCTION, DISINHIBITION AND RECOVERY

(1) Experimental extinction arises when the conditioned stimulus is repeatedly presented in the absence of the unconditioned stimulus.

This may be demonstrated when the above conditioning process has been completed. The buzzer is presented at about the same inter-trial intervals as before, but the shock is discontinued. These trials are continued until the reaction no longer occurs in response to the buzzer. Several successive failures to react may be taken as a suitable criterion of extinction. Some five or ten presentations of the buzzer alone will usually be sufficient to produce the effect.

Now, we might be inclined to misinterpret this demonstration in two ways. First, we might suppose that the 'associative connection', which was formed in the first part of the experiment, had now been destroyed. Second, we might regard this effect as somehow analogous to ordinary forgetting of, say, some skill. Both these are misconceptions as reflection and further examination will show.

(2) Disinhibition often, but not always, occurs when the conditioned stimulus of a conditioned response which has been extinguished is presented again in conjunction with a third stimulus which is novel, unexpected and vigorous. It is then often found that the conditioned stimulus will regain its capacity to provoke the response on this particular trial and sometimes on several succeeding trials when it is presented alone without the new stimulus. An attempt may be made to demonstrate this effect after the preceding extinction trials. For this purpose, we require a rather startling stimulus which the subject has no reason to expect. These three characteristics, comparative unfamiliarity, unexpectedness and violence, all seem to improve the chance of disinhibition occurring, although the effect is sometimes obtained when the subject expects that something unusual is about to happen, or when the new stimulus is a comparatively frequent event. The requirement may most easily be met by an additional auditory stimulus. For a visual stimulus of sufficient magnitude would be likely to involve so marked a change in the general illumination that observation of the subject might be hindered; and an unexpected tactual stimulus might require either additional apparatus, present from the start of the experiment, or some rather stealthy manœuvring on the part of the experimenter. The sound of a brick dropped on a tin tray, or the note of a vociferous hooter, should be suitable. It is, of course, necessary that the subject's attention should not be drawn to the presence of any such mechanism during the previous stages of the experiment. This is easily

done by foot control of the hooter. (A slap on the shoulder is also effective for disinhibition.) This disinhibition trial should be conducted immediately after the preceding extinction trial, the new stimulus being paired with the buzzer. No shock is given and the reaction is observed. If the conditioned response reappears, or does so partially, a series of extinction trials should be given until the criterion of failure to respond, which was employed in the initial extinction series, is again attained.

The frequent occurrence of this kind of effect makes it difficult to hold that the process of experimental extinction consists simply in the reduction of a previously acquired 'connection'. Again, conditioned reflex theory must have recourse to reference to the general state of the system. It must be supposed that experimental extinction has a more or less generalized inhibitory effect, rather than an influence upon the specific 'connection' established in the original conditioning. If it is further supposed that a new and vigorous stimulus temporarily dispels this inhibition, some account of these effects can be given. But these are rather arbitrary assumptions, and vague in any physiological sense. The same kind of difficulty is illustrated by two further effects which we may attempt to demonstrate.

(3) Spontaneous recovery is the restoration of a previously extinguished conditioned reflex, apparently as a simple function of time. If the subject who has undergone the above conditioning process is now allowed to withdraw from the experimental situation, and is subsequently tested once again with the conditioned stimulus presented alone, it is probable that the first few such presentations will again elicit the hand response. Within limits, the longer the rest interval, the greater is the chance of obtaining this result. A period of not less than half an hour should be allowed and an hour or more would be preferable. The subject should spend this time in a different room from that in which the experiment is conducted and should not, of course, be engaged in any similar activity. A period of a week has been successfully used, and conditioned reflexes are known to have persisted for years.

(4) A further recovery effect is revealed by a comparison of the number of trials required to demonstrate conditioning in the first place and the number required to restore it after experimental extinction. If, after the demonstration of spontaneous recovery, the

original conditioning procedure is restored until the conditioned response is again reliably obtained, it will be found that a comparatively small number of trials are required to produce once more the reaction to the buzzer alone. This 'reconditioning' process may follow on immediately after the spontaneous recovery test trials.

Again, we must bear in mind the importance of control procedures in establishing our conclusions. It might be that the subject would have made the reaction to the disinhibitory stimulus without previous conditioning and extinction. It might be, in the case of spontaneous recovery, that the subject would have responded to the buzzer after the rest interval even if some other stimulus had previously been used as a conditioned stimulus. And it might be, and probably to some extent would be, that reconditioning would be rapid when the conditioned stimulus used in the reconditioning trials was different from that used in conditioning. These and other possibilities must be investigated before we interpret our results. The control procedures involved here will be analogous to those required in the case of the original conditioning, and will often be most easily handled by employing several groups of subjects.

These effects emphasize again that extinction does not consist in the reduction of 'processes', 'connections', etc., built up during the training period. As before, within the framework of the theory of conditioned reflexes, reference must be made to the development of inhibitory conditions which prevent the operation of such connections, but do not disrupt the structures themselves. It must be supposed that this inhibitory condition is dissipated with time, thus allowing spontaneous recovery; and the case of 're-conditioning' is explained in terms of the retention of the original connections.

If extinction effects are described in ordinary terms, it is clear that what has happened is not that the subject has 'forgotten' anything, or re-learnt anything; rather he has learnt something more.* In the initial conditioning procedure, the subject 'learnt' that the buzzer was followed by the shock; although here we saw that the development of the conditioned response could hardly be taken as a fair reflection of his associative processes. In extinction, he learns that, at least temporarily, this conjunction no longer holds. But it appears that, if startled, he may behave as though it does; and if he returns to the situation after a period away from it, he is inclined to behave as though he expected the original conjunction to be restored. Finally, he has certainly not 'forgotten' his experience of the conjunction of buzzer and shock, for he has to be presented with the combination but once or twice to revert to his original response. As it were, he selects one of two hypotheses – that the buzzer will, or will not, be followed by shock – and behaves accordingly until the combination of stimuli presented to him indicates that a new phase has been entered for which the alternative hypothesis is correct. When regarded in this way, the notion of a gradual development of 'associative connections' and of their suppression by inhibition seems rather misleading; and, at the end of the experiment, the subject should be questioned about his expectations, the volitional aspects of his response, and related matters at the various stages of the experiment. [Many subjects for example have reported that ultimately their responses were 'quite automatic'.] This may indicate how far we may regard the development of the conditioned response as a reflection of the process of association of the two stimuli. And it is important to have the subject's report about the later stages of the experiment for the additional reason that if he were aware of, or guessed, the results to be expected, some might not wish to regard the experiment as demonstrating any basic learning process in a simple form.

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* Compare Freud's theory of 'forgetting' (Ed.).

We have demonstrated, then, an instance of the acquisition of a response to a stimulus, and of the inhibition of this response, of a kind which we may wish to describe as learning. But the interpretation of the experiment is complicated by the fact that human subjects do not approach such a situation without preconceptions. And we have seen reason to believe that this kind of conditioning, which is that which Pavlov's theory is directed to explain, cannot be taken as reflecting, in a direct manner, a simple associative process typical of human learning. We therefore search for some other method of investigating the properties of these hypothetical 'connections'.

DEMONSTRATION II

Many of the difficulties in the interpretation of this study of conditioned reflexes arose from the fact that we were regarding the formation of associations as something that could be created, as it were from outside, by our experimental manipulations. The subject was not given a task to perform, neither was he asked to associate to the best of his ability. This, indeed, had to be avoided. A more realistic way of studying the process of learning might be to require the subject to perform some task which involves learning, and to ensure that this task is of sufficient difficulty to expose the progress of learning to observation. When compared with the method of conditioning, the various methods by which we may attempt such studies all tend to suffer from one apparent complication. In experiments of the kind described above it is unlikely that the subject has previously experienced buzzer-shock combinations. While allowing great variability between subjects for other reasons, we tended to suppose that, to begin with, all subjects would be substantially similar in the degree to which they had already learned that buzzes are followed by electric shocks. When we set a subject some more complex task, however, it is difficult to make this assumption so confidently. If, for instance, the subject is required to memorize some passage, it is only to be expected that his performance will vary according to his previous reading, interests, and so on. And these variations may be very considerable. If we set the subject some motor skill to learn, his performance will be facilitated or disrupted according to the range of similar skills he has previously mastered. It is worth noticing that the difference between tasks of these kinds and that which faces the subject in the simple conditioning situation is one of degree only. Some subjects may have had more experience of listening to buzzers, though not in connection with electric shock, than have others. And this may affect their performance. Further, the complicating effects of previous training are not confined to experiments involving human subjects. It will be found that such difficulties also arise in almost any study of animal learning.

A partial remedy for this difficulty may be gained by the use of nonsense-syllables. (See also chapter on 'Remembering'.) Although it certainly cannot be claimed that these are quite meaningless, and hence free from previous associations, the variability introduced by this residual meaning is very much reduced in comparison with that found when ordinary words are used. Indeed, Ebbinghaus' original findings (1885; 1913. See references, chapter on 'Remembering') have for the most part fully been confirmed, although he made no allowance for the variation of 'association value' among the syllables he used. This 'association value', i.e. the average tendency for subjects to associate with a given syllable some word or meaning, has since been determined. Thus, it is possible to compile lists of syllables of roughly the same average association value. Although, often, a particular syllable will bear a special meaning for a particular subject, we may hope that this means of standardization, taken together with the use of a number of subjects and suitable control procedures, will substantially eliminate masking effects arising from the particular syllable used. On the whole, the rather extensive research which has employed this kind of material seems to bear out this expectation. (See Stevens 1951.)

If a subject is presented with a list of syllables to memorize, between what, in terms of theory of 'associative connections', are such links formed? The most simple theory would be that the links are built up between the first syllable and the second, the second and the third, and so on; or, perhaps more accurately, between the brain structures or systems which are excited in the perception of the syllables. Alternatively, it might be that the links are formed not between the stimuli, or the reception areas for these, but rather between a syllable and the verbal response which is the enunciation of the next syllable. This latter formulation has the apparent advantage of explaining how the subject learns to make the response required of him, as opposed to learning only what the next syllable is. Important considerations arise from this distinction; but they are not immediately relevant here and the notion of a 'connection', as it is now being used, is so vague that it may be doubted whether very much is gained by attempting to pass from stimulus to response at one stride. In either case, we are to contemplate a sequence in which each syllable serves as a stimulus for the reproduction of the next via some form of association between the two. This view has *prima facie* plausibility when we remember the common experience of 'losing one's place' in the recitation or instrumental playing of something learned by heart and having then to begin afresh. Two demonstrations arising from this simple theoretical framework are suggested here.

Associative sequences. According to this simple theory, the recall of any given item in a list of syllables is triggered, via a hypothetical link, by the presentation of the preceding item in the list. Hence, having learned such a list, it should not be possible to repeat the items in a changed order. For the order is regarded as an essential aid to reproduction; the learning is not of items and of the order in which they were presented as two distinct factors. This point may be tested.

Now common experience suggests that it is almost certain that if a subject memorizes a sequence of items of this kind, and is called upon subsequently to reproduce them in a changed order, his performance will fall far short of the degree of perfection exhibited at the conclusion of the original learning. But will he be completely at a loss? Does the initial learning of the list contribute nothing to the task of recounting the items in the new order? The hypothesis predicts that to learn the list in one order would impair ability to reproduce the items in a different sequence. For a given syllable will tend to evoke that associated with it in the initial learning, and hence any new association would need to be 'stronger', in some sense, in order to overcome this competition. It is not enough, however, that the subject should show little or no ability, in the early learning trials on the revised list, to reproduce the items composing it in the correct order. For it is very difficult, in any case, to say what degree of success would be expected by chance alone in this situation – i.e. if the subject were guessing. And, further, it is perfectly possible that the subject might have gained some benefit from learning the list in the original order which does not manifest itself when he is first called upon to attempt the changed sequence. Observation of the early learning trials after the change of order may not, therefore, be sufficient to test the hypothesis. Instead, the 'saving' method is used. (See also the chapter on Remembering.) The subject learns the list in one order to some criterion, say of two consecutive recitations without error. He then proceeds to learn the list in the revised order to the same criterion. His performance may be scored for both sections of the task in two ways: (1) in terms of the number of presentations of the list required to reach the criterion; (2) in terms of the total number of errors, defined as a failure to recall the item appropriate to the position in the list. A comparison of these scores for the two sections of the task will show what contribution, favourable or adverse, the learning of the first list has made to that in the revised order. The difference between the original and subsequent scores, expressed as a percentage of the original, will provide a measure of the 'saving' found.

We now require a method of presenting material to be memorized which permits a score of the subject's errors to be obtained. For instance, we might recite the list of syllables to the subject, require him to attempt to repeat them, recite again, and so on, until the list has been memorized. For this demonstration, however, the 'anticipation method' (see again the chapter on Remembering) will be more convenient. The syllables composing the list are presented for brief intervals in sequence. When the subject sees or hears the first syllable he has to respond with the second; when the second appears, he has to anticipate the third, and so on. Some starting signal is used as a cue for anticipation of the first syllable (e.g. the experimenter holds his pencil up). In this way, the subject is given equal opportunities, in terms of time, to recall each item and he finds out almost at once whether his anticipation is correct. It is found that immediate correction of this kind is an important factor in many learning tasks. In this situation, we score as an error any failure to anticipate the next syllable correctly, whether this takes the form of no response or of an incorrect response; and the subject continues until he has obtained the criterion of two consecutive error-free attempts. It is found convenient for experimenters to use slips of paper with appropriate digits to be 'ticked' for successes, etc.

If possible, the material should be presented by means of a 'Memory drum'. If this is not available, the syllables may be written on cards, which are placed regularly before the subject, each one on top of the preceding one. In this way it is possible to present each syllable for very roughly the same duration.* But it should be stressed that, even for a small scale experiment of the kind contemplated here, the memory drum is far from an unnecessary refinement. It will be found that there is a surprising degree of variation in the rhythm with which cards are placed before the subject. The task of the experimenter is complicated by the need to maintain a record of errors; and that of the subject by variation in the manner of placing, etc., which, by distraction and so forth, will introduce further variability into an already complex performance. The lists should consist of ten or twelve syllables drawn at random from a table of syllables of low association value (see Hilgard, in Stevens 1951) and they should be presented at about 2-second intervals preceded by some warning signal which may be a line, symbol, or even a further syllable which the subject is not required to remember, or a gesture, also presented 2 seconds before the first item on the list. Three such lists should be prepared. The first two consist of different syllables, and the third is composed of the same syllables as the second list, but in a changed order. We must select from the possible variations of the order, for it is feasible to investigate satisfactorily the effect of only one such change in each subject. We shall not necessarily expect all such variations to show the same 'saving'. Thus, if the list is rearranged by placing the syllables, numbered in their original order, in the sequence, 1, 3, 4, 7, 9, 11, 2, 4 . . . the effect may well be different from that obtained with a random transformation. The rearrangement 1, 12, 2, 11 . . . might have intermediate effects, or possibly be as difficult to learn as a sequence random with respect to the original list. If the experiment can be repeated, perhaps with the roles of the experimenter and subject reversed, two different orders may be compared. A short interval, of about one minute, should be allowed between each trial and the next; and a longer period, of between 5 and 10 minutes, between the completion of the learning of each list and the beginning of the next. The scores relevant to the present problem are, of course, those for the second and third list. The results will be expressed as a 'saving' value, as described above.

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* An ordinary metronome has been successfully used to time a small group.

Discussion. This procedure seems straightforward enough. If the saving score is positive, that is, the rearranged list was learned more quickly and/or with fewer errors than in its original form, we conclude that the original learning favoured the subsequent task. And hence we have grounds for doubting the hypothesis that only associations between adjacent items were learned in this task. But this conclusion is perilous. There are at the very least two control conditions about which we need to be satisfied. The reasons requiring the use of more than one subject have already been given. It might well be that, in this demonstration, the particular order of the syllables used in the rearranged list touched upon some associations already established in this particular subject, and that he therefore found it easier to learn the list in this order than in its original form. Even with the use of nonsense syllables of low association value, and with the results of two subjects, we should not feel very confident about ruling out this possibility. Second, we need to know that improvement or impairment relative to the list in its original form depended upon the use of the same syllables in the two tasks. For it is perhaps not surprising that a general practice effect is commonly found in experiments of this kind. If a subject learns a series of quite different nonsense syllables, it will be found that, up to a point, and under certain conditions, the efficiency of his learning improves with each successive list learned. Thus, any improvement found in the case of the list in revised order could be attributed to a generally improved capacity for learning of this kind, and therefore the results will not bear on the hypothesis we set out to test. Since this improvement is most marked in the early stages of practice with nonsense syllables, an initial list was learned in this experiment in order to reduce the extent of this factor. But, so to speak, this procedure is likely only to have taken the edge off the phenomenon. A satisfactory control here would require that the subject should have reached the asymptote of his efficiency. This requirement is seldom in practice assured. For different subjects will reach their asymptotes after different amounts of practice, and there is no guarantee that, after this point, performance will remain constant. Obviously, if practice is protracted, fatigue and boredom will lead to diminished efficiency; the range of nonsense syllables is necessarily limited and the use of some of them in more than one list may introduce further complications, and so on. Again, the use of many subjects is indicated. If a good deal of preliminary practice is given, inspection of their performance may reveal a point at which at least the majority are approaching a stable level. If the experiment is carried out at this point, and the difference in performance on the list in its two forms varies significantly from that between the list in its first order and the immediately preceding list, we may be more confident in our interpretation. Or, if it is found that the difference does not vary significantly from the trend exhibited in preceding practice we may reject with greater confidence the hypotheses that learning a list in one order impairs, or improves, our ability to learn it in a revised order. It should be stressed that the use of the term 'significant' here carries no special implications; we simply set a statistical limit on what we will accept as convincing evidence.

Associative links. Further experiment. A second experiment is suggested by the hypothesis of the formation of associative links between adjacent items. From this point of view there is no obvious reason to expect that the efficiency of learning would depend upon the position in the list of the items to be recalled. The association between the first two items should be established at the same rate as that between, say, the sixth and seventh syllables. This in fact is not true, as inspection of the error scores in the last experiment will probably show. But, since we wish to investigate this matter in a little more detail, it is necessary to repeat the first part of this experiment for purposes of comparison. It will be assumed that, in this experiment, the same subject or subjects will again serve.

Method. A new list of 12 syllables is accordingly presented, in the manner described before, until the criterion of learning has been attained. The same time intervals, etc., should be used, and it is particularly important here that the nonsense syllables should be of low association value. It will probably be found that the error scores show that the first and last syllables are very easy to learn, and that the other syllables are much more difficult. Most subjects will also find the second and eleventh syllables comparatively easy and the third and tenth syllables may also prove to be easier than the remainder. Thus the pattern of results obtained from most subjects, and almost invariably found when the results from many subjects are combined, shows that it is increasingly difficult to learn the syllables as the list proceeds, up to a peak of difficulty in the region of the middle to two-thirds of the way through the list; and that thereafter it becomes progressively easier to learn the items, the last syllable being about as easy as the first. Thus, if we plot the number of errors against the twelve positions in the list, we obtain a curve in the shape of an inverted 'U', with its peak somewhat displaced towards the end of the list.

As in the preceding experiment, the subject is now required to learn a second list in the same way. In this case, however, the second list is composed of different syllables, but of the same low association value. The further modification is introduced that an item in the list, placed in the region of maximum difficulty, say in the seventh position, differs from the others in some distinctive way. For instance, the seventh item may be printed in large red letters, while the other syllables are in small black type; or the distinctive syllable may be printed in the same way as the others, but be accompanied by some symbol, such as a line or a cross. We may, of course, go further and substitute a word or number for the seventh syllable, but this may require additional control procedures. With the syllable thus distinguished, this second list is learned. It will be found that the distinctive item is learned relatively easily, although performance on the first list shows that it occurs in a region of comparative difficulty. It may also be found that syllables immediately preceding and succeeding the seventh are easier to learn than their counterparts in the first list.

Discussion. It is clear that the hypothesis of association between adjacent items, in its simple form, has nothing to say about such results. It would predict neither the comparative difficulty of the middle of the list, nor that this could largely be overcome, in the case of a particular syllable, by somehow distinguishing it from the others. Before considering the implications of this finding, however, we should again review the control conditions necessary to confirm it. It seems unlikely, in this experiment, that the difference in performance in the two tasks can be attributed to any general practice effect, for here we are concerned not with overall changes in efficiency of learning, but with improvement on selected items within the task. This possibility, however, though remote, cannot be altogether ignored. For it is possible for the subject, by means of a deliberate strategy in his attempt to memorize, to produce results at least partially similar to those obtained in the second list. This will occur if the subject decides, at least for the early learning trials, to learn only the first one or two, the last one or two, and some arbitrarily selected syllables from the middle of the list. It might so happen that he chose the seventh syllable to learn in this way, and that he would have chosen the seventh even if we had not made it distinctive. It might further be that the subject would only adopt the stratagem after the not altogether encouraging experience of learning the first list. Two controls are desirable, of which only one is practicable here. If the experiment is repeated, with the roles of subject and experimenter reversed, and with the list containing the distinctive item learned first, we should be able to ascertain whether the results are due to a change in the subject's strategy as a consequence of learning a preliminary list. It will be found that this is not usually a predominant factor and that the same pattern of results will be obtained whichever order of presentation of the two lists is used. The second control procedure involves varying the position in the list of the distinctive item in a series of experiments. If it is consistently found that it is the distinctive syllable and its neighbours which are learned more easily, we may be reasonably confident that the effect depends directly upon this experimental manipulation.

We also need reason to reject the hypothesis that the difference in error scores for the distinctive syllable and for its counterpart in the first list is merely a chance variation, due to some transitory fluctuation irrelevant to the point of the experiment. As before, this is to say that we must set a criterion of how consistent the effect must be before we shall be prepared to attribute it to the experimental variable. If results are obtained from a large number of subjects, and every one of them finds the distinctive syllable easier to learn, we may draw this conclusion with some confidence. In the absence of so clear-cut a result, a statistical analysis is required.

If we wish to conclude from evidence of this kind that learning is facilitated in the region of 'distinctive' items, we have to define what constitutes 'distinction'. Will this mean certain types of stimuli: for instance a red nonsense syllable might always be more 'distinctive' than one printed in black. Or is distinction to be defined in terms of the composition of the rest of the list? In order to separate the effect of such variables, further experiments are necessary. We might compare the effect of including one black syllable among a list in red, with that of including one red among black, and so on. Generally speaking, it will be found that the effect depends principally upon the relation of the syllable to the remainder of the list and for this reason it has been described as an 'isolation' effect. Thus, if we make all the items of the list as different from each other as possible, we shall up to a point obtain greater overall efficiency of learning. But, granted individual variations, we shall still obtain the inverted 'U' curve of difficulty as a function of serial position in the list, although the effect may not be so marked. The isolation effect, on the other hand, depends upon a pronounced difference in the midst of similarity. It is clear that the phenomenon is associated with 'inhibitory' aspects of serial learning. But the term 'inhibitory' is only a name for a group of findings which seem to be similar; the explanation of such effects, whether it be in terms of interference, inhibition in some more real sense, or in yet some other form, remains to be established.

It is at least clear that the hypothesis of associative connections between adjacent items leaves much to be explained. This is not to say that there is nothing in the idea. But even these simple demonstrations show that if it is to serve at all, it must be supplemented and expressed with much greater precision. We have empirical grounds for believing that serial learning of this kind does not consist only in the formation of connections between one item and the response to the next; and also for believing that whatever kind of mechanism is involved in learning of this kind, it does not treat all the segments of such learning tasks with the same facility. It is not like a tape recorder storing with equal fidelity all the sounds fed into it. The interpretation of the results drawn from the study of conditioned reflexes also revealed difficulties in the idea of the formation of 'connections' between two stimuli. For it appeared that the unconditioned response was not simply attached to the new stimuli. Rather, some new response was learned which, while it might sometimes resemble the unconditioned response, was different from it in important respects. One of these was that it was adaptive in the sense that it was directed to prepare the subject to deal with some present or expected situation in a manner calculated to achieve certain results. These considerations prompt an examination of a further development of the 'connection' hypothesis.*

DEMONSTRATION III

We suppose that the hypothetical connections, the formation of which provides the basis for learning, are such that an association is formed between stimulus and response, rather than between adjacent stimuli. Thus, a stimulus comes to elicit a response, rather than the expectation of a subsequent stimulus. The preceding demonstrations can be interpreted in either of these ways. Within the framework of the study of conditioned reflexes the conditioned stimulus may be said to become linked either with the receptive mechanism for the unconditioned stimulus or with that controlling the response; and in the case of the learning of nonsense syllables, we may say either that one syllable is a stimulus which becomes linked to the next, enabling us correctly to anticipate the latter, or that each syllable serves as a stimulus which is connected to the response which is the enunciation of the syllable next in the list. Here, we suppose explicitly that the connection is between stimulus and response, although it may be that the very simplicity of this assumption lends it a certain implausibility.

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* It has been noted that 'nonsense syllables' were also used in the chapter on Remembering; indeed it is often difficult to distinguish between Remembering and Learning as the terms are ordinarily used.

Now, without further assumptions, this system would not in fact manifest learning; for all combinations of stimuli and responses would be linked together, and thus there would be no basis for the development of anything but a virtually random association between the two. The very nature of learning involves the selection, from a varied repertoire of responses, of some particular elements which are to be associated with particular stimuli. It is here supposed that this selection is achieved by what may be called a 'success signal'. Thus, if a stimulus is received and a response is made soon afterwards, a permanent 'connection' will only be made between the mechanisms involved if the combination is followed by a 'success signal' within some critical time. This means of selection carries with it the implication that learning will occur only when the subject is attempting to achieve some goal. Passive 'association' of the kind discussed in relation to conditioned reflexes is not allowed for; and these conditioning studies must be reinterpreted if they are not to be exceptions within the present framework. The introduction of success signals thus provides the adaptive aspect of learned behaviour, the lack of which created difficulties in the analysis of conditioned reflexes. This corollary, that learning can only occur when the subject addresses himself with some success to the mastery of a task, may seem improbable. It is not, however, obviously false and, if precisely stated, it is open to investigation.

The identification of the success signal is a complex question giving rise to much controversy. It will not be examined further here. In the absence of any decision upon this point, however, we require for experimental work a situation in which, on general grounds, we may feel reasonably confident that, even if we cannot specify the success signal with any precision, or assume that only one kind of signal is present, at least we can determine and manipulate the *predominant* incentive for the subject. It is often difficult to devise such a situation when human subjects are employed. For the goals they seek and their criteria of 'success' are extremely complex and varied. If, however, animal subjects are used, it may be possible, for instance, so to deprive them of food that we may suppose that the mainspring of their behaviour is the need for sustenance, even though many factors other than food may constitute relatively minor success signals. The following experiment, using rats as subjects, bears upon this version of the 'association' theory.

Method. On the view outlined above, learning consists in the formation of connections between stimuli and responses, contingent upon the accompaniment of a success signal. Conversely, learned behaviour will be suppressed, or altered, by the exercise of a learned response which is no longer followed by this signal. We shall attempt to investigate this point. Suppose an animal is trained to make some response, involving a clearly defined choice, in order to feed. In terms of this theory of connections, it should abandon this acquired behaviour only if, during several abortive trials, it finds that the response no longer leads to food. This prediction may be tested by so arranging matters that the rat can discover that the learned response will be unsuccessful without actually making it. If the animal is now given the opportunity to respond it should, theoretically, react as before. For only the unrewarded exercise of the response will result in the suppression of the 'connection' between it and the stimulus. A simple maze provides a situation in which such behaviour may be investigated. (See chapter 7 for general remarks about the use of animal subjects.)

An ordinary single unit maze is required to train the animal to enter one of the arms of the T to obtain food. The dimensions of the maze are not very critical. An alley-width of 4 inches and wall-height of 6 inches are suitable. The stem of the maze, and each half-section of the cross-piece, should be about 2 feet long, providing a distance of about 4 feet from the end of the stem of the T to the end of either section of the cross-piece. To each end of this cross-piece, goal box enclosures about 9 inches square and of the same height as rest of the maze should be added. The apparatus may be constructed of wood or of metal and should be provided with some form of wire-mesh roof. It must be possible to remove sections of this roof easily and silently to permit the introduction and extraction of the animals.

Two vertically sliding doors are required, placed just beyond the entrance to each section of the cross-piece. These should be oiled and fall silently upon some form of soft pad fixed to the floor beneath them. The doors will be lowered after the animal has passed beyond them and it will probably be found that this may most easily be done by suspending the doors on strings by which they may be lowered. If possible, the experiment should be conducted in a darkened room, illumination being provided by two lights of about 30 watts suspended about 12 inches above the two sections of the cross-piece of the maze. This arrangement tends to restrict the animal's attention to the inside of the maze and to be less alarming in the initial trials. But, provided lighting conditions remain more or less constant, the experiment may be carried out in normal room illumination. One section of the cross-piece of the maze, together with the associated goal box, should be painted white, and the opposite section and goal box painted black. The stem should be grey. Other major discrepancies between the conditions in the two sections of the cross-piece will, of course, be avoided – e.g. gross variations of temperature, etc. As far as possible extraneous noise should be eliminated, but it is unlikely that any marked degree of sound-proofing will be possible. It should, however, be feasible to avoid sudden, loud noises, especially while an animal is actually in the maze. The cage in which the animals are contained during the experiment should be placed symmetrically in relation to the two goal boxes – e.g. behind the stem of the maze; for, otherwise, the rat may prefer one goal box to another because this seems to afford a better chance of return to the more familiar and less frightening cage.

The training procedures required in this experiment may conveniently be divided into four stages. These are the initial 'pretraining' period in which the animals are tamed and become accustomed to the kind of apparatus in which they will subsequently be trained; the 'training' period, when the rats will be taught to find food in one of the goal boxes; the 'latent extinction' period, during which the animals will be placed directly into the goal box in which they have previously fed and find that food is no longer present; and, finally, the 'extinction trials' in which the animals run the maze as in the training period, but obtain no reward whichever choice is made. How much of this experiment can be completed, and the extent to which previously trained animals are used, will depend upon the time available. The crucial part of the demonstration arises, of course, in the last two stages, both of which are conducted on the same day. But it is very desirable, both on general grounds and for practical reasons concerned with the handling of the animals, that the training stage should also be carried out by the same experimenter, and this requires a period on each of the two days preceding the extinction day. The pretraining is a crucial preliminary to most animal experiments, and, in this case, requires a rather short period of time on the five days preceding the first training day. The methods applied in all four stages will be outlined briefly below.

Four rats are required. They should be males and about four months old. The method of housing will depend upon the facilities available, but it will suffice, and may indeed be an advantage for the purposes of this demonstration, if they are maintained together in one cage.

Pretraining. This should be carried out in the room in which the animals are subsequently to be trained and, as far as possible, in roughly the same position. The rats' cage should be placed in the position which it will subsequently occupy and lighting and other conditions should be roughly similar to those which will be encountered later. The rats will be hungry, the run taking place about 4 hours before the usual feeding-time. If circumstances permit, all stages of the experiment should be conducted at about the same time of day. The animals are placed singly on a table, upon which there are several small heaps of food pellets, for about 10 minutes. It will be found that they will be reluctant to eat at first; but, after one or two days, they will run directly to the food. Each animal should be lifted two or three times during each daily period on the table and replaced in a different position. When all four rats have been treated in this way, they should be allowed food in their cage for about 2 hours. The procedure is repeated on each of the five days devoted to pretraining.

Training. The maze is set up as described above. In one corner of one of the goal boxes, invisible from the choice point of the maze, a small pile of food pellets is placed. For two of the rats, Pair 1, the food should be placed in the white goal box; for the other two, Pair 2, food will be found in the black box. Each animal is given a total of ten trials each day, run alternately within each pair, that is, the first animal of Pair 1 has his first trial, the other member of the Pair his first trial, and then the first member runs for the second time, and so on. Pair 2 is trained after Pair 1. On each trial, the two doors beyond the choice point are raised, the animal is put into the end of the stem of the maze and the appropriate door is lowered after the rat has passed beneath and well beyond it. Great care should be taken not to drop the door when the rat is within a few inches of it. For, even if the door does not actually touch the animal, such an experience may be sufficient to establish a very persistent avoidance of this alley in future trials, or, possibly, to induce a state of immobility for the rest of the experiment. The rats will be hungry as in the pre-training stage, and should be allowed about 30 seconds in which to eat when, on correct trials, they choose the side containing food. This time is measured from the moment at which the door is dropped behind the animal and, when an incorrect choice is made, the rat is detained in the empty alley for the same period. Each rat should be given ten such trials on both of these two days, being retained in the cage with the other animals between trials. If possible, training should be continued for a third day; but, if this is impracticable, and the animals have made few or no errors on the second day, two days will suffice. The time taken to make a choice, i.e. from the moment of entry into the maze until a door is dropped, is recorded for each trial. Errors are similarly recorded.

According to laboratory and other convenience the instructor will decide how far members of the class are to be used both for training and for final demonstration. A large mirror can be arranged over the maze so that quite a sizable class can see what is going on. This arrangement has the advantage that class 'watchers' are less likely to disturb the experiment.

Latent inhibition. Both this and the following 'extinction' phase of the experiment are conducted on the day following the last day of training. Only one member of each pair is used at this stage. There should, at the end of the preceding training period, have been little difference between the performance of the two members of each pair. And, indeed, if one member was markedly superior to the other, the point of the demonstration is lost. It is probable, however, that some difference in performance will be found, and that rat which made the smaller number of errors on the last day of training should be selected for use in this stage of the experiment. These rats are placed by hand directly into the goal box in which they have previously been rewarded. On this occasion, however, and for the rest of the experiment, food is removed from the maze. The doors separating the goal alleys from the stem of the maze are closed, so that the rat is unable to leave the empty goal box and section of alley. The animals are retained in the goal box for about 2 minutes. As before, the trials are conducted alternately, so that the first rat spends 2 minutes in the black goal box, the member of the other pair is then placed in the white box, then the first is returned to the black box, and so on. Each rat should be so treated four times, returning to the cage between trials. The experiment then proceeds at once to the last stage.

Extinction. Here all the rats are once again employed, and ten trials are again run in exactly the same manner as in training, the only difference being the absence of food in the maze. It may be found that after several such trials some of the rats will no longer leave the stem of the maze for either of the two arms. It is suggested that a time limit be imposed in such cases after which the animal is removed from the stem. This limit is best determined in the light of the behaviour of the rats at the conclusion of training; it will probably be found that about 2 minutes is an appropriate criterion. The time required for a choice is recorded as in training, together with a score of errors. When an animal fails to make a choice, the trial is scored separately, and the latency of the response is taken to be the criterion value. (See also chapter 7, 'Experiments on Animals'.)

Discussion. A comparison of the performance of the rats which experienced the 'latent inhibition' trials, with their counterparts which did not, should reveal whether experience of the empty goal box, without the exercise of the responses normally required to reach it, tends to hasten the ordinary process of extinction. Four scores may be compared: (1) number of entries into the incorrect – i.e. previously unrewarded – arm in the extinction trials; (2) number of refusals to make a choice within the time limit imposed; (3) these two scores combined – i.e. a refusal is counted as an error; (4) latency of choice. It is usually found that the animals which have not experienced the empty goal box persist in unrewarded runs to it longer than do animals which have previously found it empty. This result is amply confirmed in experiments on a larger scale.

Again, in the interpretation of this finding, we must refer to necessary control conditions. We wish to establish that a difference found in the behaviour of the two groups of animals is due specifically to the experience of an empty goal box. Hence, it is necessary to rule out at least the alternative hypotheses that the 'latent inhibition' animals would in any case have been more subject to extinction; or that, although this more rapid extinction was due to the different experience of the animals before the extinction trials, the relevant aspect of this experience was, for instance, that of being handled, or of being placed directly in the food chamber. The evaluation of such hypotheses as these requires the use of a larger number of animals. The first possibility may be countered to some degree by so selecting the animals that those allotted to the 'latent inhibition' group show a performance roughly equivalent to that of the control group during the initial training stage. It will be arranged that any difference that may exist between the performance of these two groups should be unfavourable to the hypothesis of 'latent extinction'. In this case, an attempt of this kind has been made on a small scale by forming the 'latent inhibition' group from the two animals which showed the better learning in the training stage. It may be presumed that, other things being equal, these rats would extinguish more slowly; or, at least, we should not expect them to extinguish in a smaller number of trials than their more backward counterparts. This assumption, however, that animals which learn more quickly do not also extinguish more quickly, requires confirmation to be obtained only from further studies in similar situations. Certainly, it cannot be assumed to be correct of necessity; and hence the fairly close matching of the two groups in the training stage, requiring the use of a sufficient number of subjects, is of very great importance. The elimination of the second possibility, that the difference in performance between the groups is attributable to differences in handling, etc., requires the employment of additional animals, differently treated. Thus, we may use groups which are lifted from their cages and returned to them, but which are not, as are the rats in the 'latent inhibition' group, placed in the empty goal chamber. Another group may be placed for an equal time in a new and distinctive box; and yet a third group placed in the goal chamber of the maze with food present as in the training stage. From a comparison of the behaviour of such groups, we should be able to determine, with reasonable confidence, how far the behaviour of the original experimental group was due to 'latent inhibition'.

These demonstrations have been directed to show that the simple notion of the formation of 'connections' between stimuli and responses is inadequate at least on the grounds: (*a*) when, as in conditioning, it appears that a response is 'connected' to a stimulus this does not consist in the transfer of the identical response from one stimulus to another; (*b*) in sequential learning, the learning process cannot be entirely accounted for in terms of the formation of 'associative connections' between one item and the next, or the response which anticipates the next; (*c*) since not all stages of a sequential learning task are equally easy, and since the difficulty of any point in the sequence is determined in part by the characteristics of that stimulus relative to the neighbouring stimuli, at least some addition to the simple account in terms of 'connections' is required; (*d*) since extinction can occur without the exercise of the response extinguished, we must allow cases of learned modifications of behaviour which cannot easily be expressed in terms of a variation of strength of a 'stimulus-response connection'. One further aspect of the learning process will provide the subject for a less formal demonstration.*

DEMONSTRATION IV

The learning tasks employed above have all been such as to permit the gradual development of behaviour to the point at which performance meets some criterion of mastery. In all cases it might have happened that the process of learning took the form of a more or less extended period of little or no improvement followed by a comparatively abrupt improvement to near perfection. But, in such tasks, this is not usually found. Some learning situations, however, are so arranged as to tend to enforce this more discontinuous development of learning. These are sometimes distinguished from ordinary cases of learning by the name 'problem-solving' (q.v.). This distinction raises complex issues and the grounds for the distinction are often obscure; it is, however, clear that there is a range of situations which may be expected to favour discontinuous changes in behaviour, but which nevertheless involve 'learning' in the sense that some experience of the task or problem enables the subject to do something which he could not hitherto achieve. An example of such a task is that in which the subject is presented with a series of objects which he must sort into two or more categories. The principle of classification is unknown to him and his task is virtually to discover this by inference from previous successes and failures. We may expect that his performance will show little improvement until he discovers the correct principle; thereafter, his selections should almost invariably be correct. Such tasks are commonly said to involve 'concept formation'. But we shall not here inquire into the justification for, or indeed the meaning of, this description.

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* Demonstration 3 has been concerned with the general question of the extent to which learning may profitably be regarded as a matter of the formation of 'connections' between stimuli and responses; and with the difficulties arising in the control and interpretation of results of this kind. If adequate control procedures are employed, it becomes possible to state and investigate the problem within the context of more detailed alternative theoretical formulations. An outline of some of these is given in Chapter 7, where a similar experiment is directly related to such theories.

Method. The class is divided into pairs, each with a subject and an experimenter. The subject is presented with a sequence of cards upon which are displayed patterns or other combinations of cues, so arranged that the cards may be sorted into two categories on the basis of some principle. Any rule and material will serve our purpose here, provided it satisfies certain conditions. The application of the principle to the material must be unambiguous, the material should be such that we may be reasonably certain that no other rule yields results co-extensive with those derived from that the subject is required to discover, and the task should be fairly difficult. The subject may, for instance, be required to select, from a sequence of meaningless and rather complex geometrical drawings, those which contain the figure '4'. It will be found that the addition of a very few lines to the '4' will camouflage it effectively. Alternatively, we may present simple geometrical figures which vary in size, orientation, colour, number of individual lines forming each main contour, etc., and in this case the principle required will specify some combination of conditions – for instance, the combination of all contours the same colour and the presence of at least one right angle. A fairly long sequence of, say, 150–200 such 'cards' should be prepared and duplicated. These are presented singly to the subjects so that they are available for inspection for only a short time, perhaps 5–10 seconds. At the beginning of the experiment, the subject is told that he will be presented with a series of designs, some of which will be of type 'A' and some of type 'B'. His task is to allocate them to the two categories as correctly as possible. When he can see no grounds for allocation, he must still guess the type of the design, at the latest when the card is withdrawn. When, on the other hand, a judgement is based upon some hypothesis about the difference between the two types of design, he should state this hypothesis when he first applies it. The success or failure of each judgement will be reported to him by the experimenter, who is given a duplicated 'key' for the purpose.* A record is kept of the subject's successes and failures and also of the 'hypotheses' he reports and the stages in the sequence of judgements at which these are employed. A short interval to reduce fatigue, etc., should be allowed after each block of 20 judgements. If the subject does not solve the problem when the original supply of designs have all been used, the experiment may be continued by presenting the same designs again in a different order. For it is unlikely, with this kind of material, that the subject will be able to remember how, and with what success, he originally allocated the cards. But the point at which such repetition begins should be noted and the results should subsequently be scrutinized for any effect that repetition may have had. If possible, the experiment may be carried one step further after the subject has discovered the 'correct' hypothesis. The procedure is continued unchanged for a further fairly long sequence of designs, say 20 or 30, all of which will, of course, be allocated correctly; the subject is not told, however, that this 'hypothesis' is correct. At the end of this sequence, the principle determining correct allocation is changed, without the subject's knowledge, so that he once again finds himself in error.

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* It is suggested that the 'keys' should be collected and retained by the instructor.

The experiment proceeds as before for as long as possible, or until the subject discovers the revised principle.

Discussion. Our first interest in the analysis of the records derived from this experiment concerns the shape of the learning curve. There are two possibilities.

Since the solution of the problem depends upon the application of the correct principle, a rather long period of little or no improvement should be followed by a relatively abrupt transition to perfect performance. This change should coincide with the enunciation of the correct principle. The results for each block of twenty trials may be expressed in terms of the percentage of correct judgements for that sequence. These values should be plotted against the series of blocks of trials; inspection of the learning curve so formed will reveal how far the expectation of a discontinuity in the subject's performance is confirmed. Such discontinuous curves are obtained fairly frequently.

But, depending upon the subject and the type of material used, a more gradual approximation to complete mastery of the problem is often found, and performance then shows more than chance success for a lengthy period before the correct hypothesis is pronounced. If, as is more probable, this second result is obtained, we must suppose either that the subject was at this earlier stage working in accordance with a principle which, although not the correct one, still yielded results which were more often right than wrong, or that he was behaving in accordance with such a principle unwittingly. The records of each experiment should be analysed to determine which of these alternatives holds at the various stages of the sequence. Thus, each principle announced by the subject is compared with his allocations in the trials following that judgement, to determine how far his subsequent performance is in fact in accordance with the principle and how far the application of the rule would yield the degree of success obtained. It will probably be found that the pre-solution period contains (1) some sequences in which the subject is only guessing, and in which he achieves only that degree of success to be expected by chance alone, (2) some periods in which he is consciously applying a principle which affords better than chance performance, and (3) some in which his judgements are correct to an extent which cannot be completely accounted for in terms of the principle upon which he was consciously working at the time. There may also be periods during which the subject is much more frequently correct than we should expect by chance, at times when no explicit hypothesis at all is being used.

The results of the second stage of the experiment, after the change of rule, should be examined separately. We are concerned here with the effects of a prolonged series of successes upon the subject's ability to continue his search for principles which are even more successful. Logically, only one failure should be sufficient to show that a rule is inadequate. This should then be abandoned in favour of some alternative. This in turn should give rise to a sequence of judgements which achieve only chance success; or alternatively, the subject may continue to apply the incorrect rule, for want of a better, until he thinks of another. We may therefore examine the record of this second stage with two questions in mind.

First, did the subject continue to apply the previously successful rule, or did he abandon it at once? Second, in either case, were more trials required to elicit the use of a different rule than had usually been needed in the preceding part of the experiment? It is often found that subjects are less willing to abandon the use of an hypothesis which has been consistently successful in this way; or that, even if they do abandon it, they are retarded in the construction of the next principle to be tested. A comparison between the number of presentations required, in this part of the experiment, to induce a change with the mean number required to elicit similar changes in the case of several hypotheses preceding the adoption of the first correct principle, will indicate whether the subject was unusually reluctant to abandon a successful rule. Similarly, a comparison of the number of presentations intervening at this stage between the point at which the principle was changed and that at which the subject announces a new rule with the mean number required for the corresponding shifts before the selection of the first correct principle, may suggest that the subject is retarded in the formation of a further rule. The two results taken together will indicate whether any such retardation is due mainly to the subject sticking to his original hypothesis, or whether he finds additional difficulty in the formation of a new principle, even after he has abandoned the first rule.

As before, the interpretation of the results of demonstrations on so small a scale must be treated with great caution. It is found that the results of experiments of this kind are very dependent upon the subjects, upon the material used and the difficulty of the task, and upon the precise form of the instructions given to the subjects. For instance, in the interpretation of the form of the initial learning curve it would of course be unreasonable to generalize from the results of one subject. Again, the curve derived from the performance of any one subject is likely to be rather irregular for reasons irrelevant to the investigation. Thus, important features of the results such as, for instance, the effect of considerable partial success of a wrong hypothesis, may be masked until the results of several subjects have been combined. On the other hand, care must be taken, in combining results from several subjects, not to assume that there necessarily is only one fundamental pattern of behaviour exhibited in this kind of task. Rather, it is more likely that there are at least several different basic strategies involved. Of two different subjects, or the same subject at different stages of the experiment, one may simply allocate the designs at random when there is no hypothesis to test at that time, and the other may allocate according to some hypothesis which he already knows is incorrect. Hence the records of the subjects should if possible be scrutinized individually to detect consist patterns of behaviour which are typical of all or some subjects at the different stages of the experiment. The use of a number of subjects is also required in order to discriminate systematic effects from chance variations.

Bearing in mind the need for such precautions, the results of this demonstration may suggest that, despite the apparent all-or-nothing nature of the learning task confronting the subject, the behaviour exhibited cannot be adequately characterized in terms of a period of no improvement followed by an abrupt change to wholly correct allocations consequent upon the discovery of a correct hypothesis. Performance tends to be less discontinuous, showing lengthy periods of partial success before the point of complete mastery. This partial success will probably be found to depend upon the use of principles which yield above chance performance and which, by a process of modification, approximate gradually to the final correct rule. It may also have been found that the subject's performance showed improvement at times when he could not consciously formulate the principle upon which he was working. Finally, it is likely that the change of rule after continued success will have presented particular difficulty. Thus, most subjects, at least at some stage of the experiment, behave in a manner more analogous to the gradual learning process observed in preceding demonstrations rather than in that to be expected if the subject was testing a series of hypotheses in a strictly logical manner.

Conclusion. These demonstrations may serve to illustrate two main points. First, it seems unlikely that any theory of 'associative connections' in a simple form will prove satisfactory; and it is not at all clear whether, if learning is to be analysed in terms of the formation of connections at all, these should be conceived as linking one stimulus with another, or as linking stimuli with responses. Indeed, it may be that an adequate theory will require both conceptions in some form. On the other hand, the results of the last demonstration suggest that there may be a greater degree of continuity between simple and more complex learning, involving the formation of 'concepts', than might be expected *a priori*. Second, the results gained from any single experiment in this field can only be interpreted unambiguously if rather complex control conditions are satisfied. This, of course, is the natural consequence of the inevitable complexity of the experimental situations involved. It cannot be stressed too strongly that the results of any such experiment are susceptible to many explanations, quite apart from those that are, as formal predictions, drawn from some theoretical analysis. But this is not to say that control experiments cannot be devised to eliminate or confirm these alternative explanations; it does, however, imply that the confirmation of a theoretical prediction requires experiments on a larger scale, and under more various conditions, than might at first have seemed necessary. It is usually idle to search for a short cut in laboratory investigations on learning.

REFERENCES

General

BUGELSKI, B. R. (1956). *The Psychology of Learning*. London: Methuen.

HOVLAND, C. I. (1951) 'Human learning and retention', in *Handbook of Experimental Psychology*, ed. Stevens, S. S. New York: John Wiley.

OSGOOD, C. E. (1956). *Method and Theory in Experimental Psychology*. New York: Oxford University Press.

WOODWORTH, R. S., and SCHLOSBERG, H. (1955). *Experimental Psychology*. London: Methuen.

Demonstration I

HILGARD, E. R., and MARQUIS, D. G. (1961). *Conditioning and Learning*. Revised by Kimble, Gregory A. London: Methuen.

PAVLOV, I. P. (1927). *Conditioned Reflexes*. London: Oxford University Press.

Demonstration II

HOVLAND, C. I. (1938). 'Experimental studies in rate-learning theory, III: Distribution of practice with varying speeds of syllable presentation'. *J. exp. Psychol.*, **23,** 172–90.

McGEOCH, J. A. (1942). *The Psychology of Human Learning*. London: Longmans.

PILLSBURY, W. B., and RAUSH, H. L. (1943). 'An extension of the Köhler-Restorff inhibition phenomena'. *Amer. J. Psychol.*, **56** 293–8.

RASKIN, E., and COOK, S. W. (1937). 'The strength and direction of associations formed in the learning of nonsense syllables'. *J. exp. Psychol.*, **20,** 381–95.

STEVENS, S. S. (1951). *Handbook of Experimental Psychology*. New York: John Wiley, 53.

ZIRKLE, G. A. (1946). 'Success and failure in serial learning, II: Isolation and the Thorndike effect'. *J. exp. Psychol.*, **36,** 302–15.

Demonstration III

MOLTZ, H. (1955). 'Latent extinction and the reduction of secondary reward value'. *J. exp. Psychol.*, **49,** 395–400.

LEWIS, D. J., and COTTON, J. W. (1958). 'Partial reinforcement and none-response acquisition'. *J. comp. physiol. Psychol.*, **51,** 251–4.

SEWARD, J. P., and LEVY, N. (1949). 'Sign learning as a factor in extinction'. *J. exp. Psychol.*, **39,** 660–8.

Demonstration IV

DREVER, J. I. (1934). 'The pre-insight period in learning'. *Brit. J. Psychol.*, **25**, 197–203.

LEEPER, R. (1951). 'Cognitive processes', in *Handbook of Experimental Psychology*, ed. Stevens, S. S. New York: John Wiley.

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Chapter 5

MOTOR PERFORMANCE

HARRY KAY
Sheffield University

J. SZAFRAN
Exeter University

The actions of human beings are so varied and complex that they are an obvious field of study for the psychologist. Curiously enough, the problem has not been investigated with anything like the intensity it deserves and for long during the 1920's and 30's surprisingly little attention was paid to it. It is only during more recent years that any significant attempt has been made to follow up the original work in this field which was done around 1900 by such men as Bryan and Harter (1897), Breitwieser (1911) and Woodrow (1914).

Flexibility. Let us begin by considering typical actions of an adult – his ability to walk or run, to pick up an object, to throw it accurately and to catch it when it is thrown at him. Several features are predominant. Firstly these performances are extraordinarily flexible. Running speed and style can be adjusted to terrain, a hand may use a pick or a needle, a shovel or a pair of tweezers, a ball can be caught from every sort of angle or trajectory. To stress this flexibility another way, it would be comparatively simple to design a model which might perform any one of these operations, but extraordinarily complex to design it so that it could carry out this range of tasks.

Learned co-ordinations. A second feature of such accomplishments, closely linked to the first, is that they have been learned. A baby cannot walk and only does so after the usual number of trials and bumps. Early attempts to thread a needle, tie a shoelace, throw a stone, emphasize that we have had to acquire the very necessary skilled co-ordination even in such workaday activities.

Timing. A third feature, which is revealing in high speed skills, is the smoothness and unhurried grace with which it is conducted. Bartlett (1948) in particular has pointed out how we talk about expert games-players having 'all the time in the world' to make their strokes. In spite of the speed at which a ball is thrown, they do not appear to be hustled by it; that is to say, the external event may take place so quickly that there is only the minimum of time in which to perceive and initiate a response to it, yet somehow the skilled player does manage to make the appropriate response and in everyday talk we 'explain' his success by attributing it to anticipation. This begins to look as if he is in the right place at the right time, not because he can respond quicker to the signal but because he began to respond earlier in time, perhaps before the signal did in fact occur. The problem for us is to say not only how he succeeds in anticipating correctly, but *why it is necessary to do so*. In the world of electronics we have become accustomed to receivers which respond to their inputs with near simultaneity. The human nervous system, though transmitting electrical impulses when peripheral receptors are stimulated, is evidently not of this kind. It is then necessary to measure what are the basic response latencies of the organism. These will not tell us how the organism will behave in a series of circumstances, but they should tell us what it has to 'overcome' in a skilled activity. As might be expected, we shall find that many of the activities which are labelled skilful are so designated because in them some of these apparently basic limitations of the human system have been successfully circumvented.

In this chapter we shall discuss experiments which throw light upon some of the characteristics of human performance. These studies may give us some hint as to the kind of system represented by the human organism. The first group of experiments – reaction-time studies – on the ability to respond to discrete signals examine some of the inherent limitations of the system.

I. REACTION TIME (RT) STUDIES

In the classical reaction time study everything is done to facilitate the performance of a subject. The aim is not to try to catch an individual in an unsuspecting moment but to find out how quickly he can make a response to a signal under optimum conditions. A typical situation for examining visual reaction times is as follows.

A subject is seated in front of a screen in which there is a small aperture for a neon-light-bulb. His hand rests on some form of electrical switch, often a morse key. He is instructed that, as soon as he sees the light appear, he is to respond by depressing the key. The apparatus is so constructed that the experimenter completes the circuit when he switches on the light and thereby starts an electrical clock. The subject breaks the circuit with the action of his key, and the clock measures to the order of 1/100 or 1/1000 of a second the interval between the onset of the light (stimulus) and the key response – the S-R interval.

Piéron's experiment. The apparatus requirements are not complex but they do demand a precision timer and a stimulus with a quicker onset than the ordinary electric bulb, whose filament takes a few milliseconds to illuminate. Piéron (1928), also reported by Woodworth and Schlosberg (1954), suggests a delightfully simple procedure requiring little apparatus. The experimenter holds with his thumb a metre rule in a vertical position against a smooth wall. The lower end of the rule is placed against an index mark on the wall at about eye level. A subject has his hand at this mark and is ready to stop the fall of the rule by pressing his thumb against it. The experimenter says 'ready', and then suddenly releases his thumb, allowing the rule to fall. As soon as a subject sees the experimenter's thumb move, he jabs his own thumb against the rule. The distance the rule has fallen is easily read off the scale at the index mark, and this distance can be converted into a fairly accurate time measure by using the formula $T = \sqrt{\frac{2S}{G}}$, where T is the time in seconds, S the distance of the fall in centimetres and G the acceleration due to gravity, 980 cm/sec. If a time scale is drawn on the rule this further simplifies the procedure. The formula reads $S = \frac{1}{2}GT^2$ which gives the following values.

RT (sec)	·10	·15	·20	·25	·30	·35	·40	·45
Fall (cm)	4·9	11·0	19·6	30·6	44·1	60·0	78·4	99·2

(from Woodworth and Schlosberg 1954)

If an auditory RT is required the experimenter can press upon the rule with a 'clicker' and conceal its view from a subject.

Central and peripheral latencies. To return now to more theoretical considerations, it will be noticed that the RT is not measuring a movement or response time so much as the time to *initiate* a response – it is a latency measure. Several different stages can be distinguished as contributing to this total measure, such as the time taken by the peripheral sense organ, the conduction time of the nerve, the latency of the muscles and the time required for transmission within the brain itself. With visual stimuli the retina, because of its comparatively slow 'photo-chemical' processes, may take as long as 20–40 ms (milliseconds) before any signals are transmitted up the optic nerve to the visual cortex. With auditory stimuli, the corresponding latency for the ear is only of the order of 1 to 2 ms. Other things being equal the auditory RT should be shorter than the visual and in fact the usually quoted figures for the total RT are 140 ms for audition and 180 ms for visual.

But the latency of the peripheral sense organ only accounts for a fraction of the total RT, whilst the rate of nerve conduction both to and from the brain is so fast that it too contributes only a few ms. For audition, impulses reach the cerebral cortex within less than 10 ms from when the sound reaches the ear, whilst direct stimulation of the optic nerve, which by-passes the retina, has a cortical latency of around 2–5 ms. Some 10–15 ms have also to be allowed for conduction on the outgoing side from the brain to the muscles controlling the finger, and, more important, some 20 or 30 ms may elapse before the relevant muscle group is sufficiently energized to make a response such as depressing a key. This of course depends partly upon how much pressure is required to move the key. Where it is set as lightly as possible, as it should be, this latency is reduced but even so the RT as measured by the time when the key is depressed is at least 20 ms longer, and in many instances 30–40 ms longer, than the same RT measured by recording action potentials from the relevant muscles.

It would seem that for visual RT's, peripheral time may account for some 60–80 ms, and for audition some 35–45 ms, leaving a central RT of approximately 100 ms. In other words, the major part of the RT is taken up by processes within the brain itself and which for want of a better name we may call *analysis time*. It is by no means clear why this analysis time of the brain is so long, though, with the wisdom of hindsight, it is easy enough to attribute this to the intricate complexity of the neuronal circuitry of the brain with its astronomical number of cells and connections. But the significant feature from a behavioural standpoint is that the simple RT for a normal adult subject very quickly reaches a minimum value and thereafter is not materially influenced by practice. In other words, however much a subject may be prepared for what he has to do in a particular situation, it is not possible for him to 'short circuit' this lengthy operation. As already noted, human skilled performance may be extremely flexible, but the price of flexibility is a correspondingly complex mechanism with its accompanying longer latencies.

Foreperiod variation. We have already observed that the practice effect with simple RT is not marked. It is customary to note that the RT duration does decrease with increasing intensity of stimulus, though in fact once the stimulus is clearly above threshold this effect too is slight. A more important variable is the foreperiod, that is, the time elapsing between the subject receiving a 'ready' signal and the onset of the actual stimulus. There are several different procedures. An experimenter may use an *irregular* foreperiod, but specify the range within which his signal will occur, e.g. not less than 1 second and not more than 9. Within this range he may have a rectangular distribution of intervals. It is generally stated that the optimal foreperiod is around 2 seconds. It is for practical purposes a most convenient foreperiod for an experimenter to use but the optimal is determined by the range of times which is being employed.

When a fixed foreperiod is used the subject knows when the stimulus will occur and to prevent false anticipatory reactions the convention is to use 'catch' trials: that is, on a certain number of trials to give the ready signal but not to follow it with the actual stimulus. This is an effective check but it should be noted that what is being done here is to reduce the probability of the signal from certainty by a prescribed amount. Hence it is important to note the proportion of check trials which is used. The reason why this influences reaction time may be clearer when we have discussed the uncertainties introduced in choice reaction studies.

Choice reaction times. So far we have limited our considerations to instances when a subject knows that there is only one possible signal, but there are many circumstances where, though only one signal occurs at any one time, it may be one from any number. This situation is known in the laboratory as a *choice reaction*. The query arises whether, when there are two possible signals, a subject takes longer to respond to one of them than he would take in the simple reaction situation. In fact he does take appreciably longer; the 2-choice RT is of the order of 300 ms which is approximately 50 per cent longer than the simple RT.

It is by no means obvious why this is so. A subject is responding to one light when it appears and it might be argued that this positive response is not influenced by what might have occurred, but only by what in fact has happened. Results show that this is not so. Apparently the organism has the ability to prepare two possible responses to two possible signals, but to accept either signal and trigger off the appropriate response takes appreciably longer than is the case with the simple RT. Classical experiments such as Merkel's (1885) found that the RT increased as the number of choices increased, though it will be observed that the major step is the difference between the simple and 2-choice situations.

TABLE XIV. Data from Merkel (1885)

Number of alternatives	*Reaction time*
1 (simple reaction)	187
2	316
3	364
4	434
5	487
6	532
7	570
8	603
9	619
10	622

The table gives the averages for nine subjects. (After Woodworth and Schlosberg 1954.)

Hick (1952) repeated Merkel's experiment, confirmed his findings, and suggested an explanation based upon the concept of 'quantity of information' as developed by communication engineers. Before presenting Hick's explanation it will be well for us to consider some of the more relevant features of information theory since it has influenced much recent research upon skills.

Information theory. Information theory assesses the amount of information of a particular signal in terms of its uncertainty. Where one event x occurs out of a possibility of x and y, it carries less information than if the possibility had been x, y and z. Where an event is extremely improbable it carries much information, that is, its occurrence resolves much uncertainty. Conversely, if the event is extremely likely it carries little information. Where we can state accurately the probability of an event, as in RT experiments, we can calculate the amount of information per signal, e.g. if event i has a probability p_i, the amount of information is $I = - \log p_i$.

In practice we deal less with a particular event than with the sources of a number of events. Particular events may contain different amounts of information but what is required is the average amount of information to be expected from a source. This extends the above formula to –

$$H = \text{mean value of } (- \log p_i)$$

$$= \sum_{i}^{n} p_i \log p_i$$

The unit for measuring the average amount of information is the 'bit' (binary unit). It is a ratio measure. Where two events are equiprobable we need one bit of information to decide between them; that is, every time the number of alternatives is halved one bit of information is gained. Thus with eight possibilities we need in all three bits to decide on a particular event; one bit to decide which four of the eight possibilities, plus one bit which determines two of the remaining four, plus one bit to determine which one of the final two alternatives. For convenience, information theory usually employs logarithms to tne base 2, which gives an information content of 1 when $p = {\cdot}5$.

Rate of gain of information. At first sight it seems odd that information should depend solely on a probability and this usage is of course quite different from that which associates information with context or meaning. But the statistical advantages are considerable as can be seen when we return to Hick's experiment. He put forward the hypothesis that 'the rate of gain of information is, on the average, constant with respect to time, at least within the duration of a single perception . If then the hypothesis is correct the increased reaction time for the multi-choice signals will be proportional to their increased amount of information. Hick found this to be so for both Merkel's data and his own. He had to make one assumption, that since log n only measures the uncertainty as to which stimulus will occur, some constant must be added to allow for the doubt about when it will take place. Hick proposed that 'the possibility of no stimulus' was treated by the subject as if it had the same probability as any particular stimulus. This makes the gain in information equal to log $(n + 1)$. Applying the formula RT (seconds) $= \cdot 626 \log_{10} (n + 1)$ to Merkel's data he obtained a very close fit. It will be observed that there is an implicit assumption that the movement time is constant from one condition of choice to another and further that all signals represent the same level of perceptual difficulty. The following experiment which demands the minimum of equipment provides a simple method of investigating the relationship between the number of choices and the time taken.

EXPERIMENT I. A STUDY OF CHOICE REACTIONS USING A CARD SORTING PROCEDURE

The only apparatus requirement is one or two packs of playing cards. The method, originally designed by Crossman (1953), is by comparison with the classical method of investigating choice-times much simpler and more flexible, though it sacrifices the keeping of a detailed record of individual responses whilst movement time is greater than in the more customary RT procedure. However movement times for card sorting prove to be sufficiently constant for this to be only a minor objection.

Since the task of sorting and dealing cards is familiar to most subjects, very little practice is required. A subject is allowed to familiarize himself with handling the particular pack and the experiment can begin.

Balanced frequencies. A subject is instructed to sort the cards into the classes as given for Tasks BI, BII and BIII in Table XV. Where there are 2 classes only the two colours, red and black, are to be differentiated; for 4 classes the suits; for 13 classes the numbers. The pack is held face downwards to prevent preview of the next card. Each card is to be 'dealt' individually and to ensure that the first sorted card is put in the correct place appropriate sample cards of each class should be displayed at the relevant positions. The instructions are to work as fast as possible trying to make no errors and not correcting any if made. Trials in which there are more than 10 per cent misplaced cards are discarded. It is intended that a subject should adjust his speed to the maximum possible; it is better that he should be working fast and make a few errors than proceed too slowly in order to ensure an error free performance.

The total time taken to sort the pack is recorded by a stop-watch to the nearest convenient figure. At each stage some practice effects are likely to take place before a subject settles down to what he regards as a comfortable rate. It is instructive to compare the rates of improvement for the different tasks, but for the purpose of calculating the average sorting time per card for each class it is necessary to have a stable performance. A satisfactory measure is achieved by considering within each class the first three trials showing less than 5 per cent variation. From these data the relative sorting rates may be plotted.

TABLE XV

Balanced frequencies

Task	*Number of classes*	*Criteria*
BI	2	Red/Black
BII	4	Hearts/Diamonds/Spades/Clubs
BIII	13	A, 2, 3, 4, 5, 6, 7, 8, 9, 10, J, Q, K
		Unbalanced frequencies
UBI	2	Picture cards/Plain cards
UBII	4	Red pictures/Red plain/Black pictures/Black plain
UBIII	8	Hearts pictures/Hearts plain/ Diamonds pictures/Diamonds plain/ Spades pictures/Spades plain/ Clubs pictures/Clubs plain
		Estimates of movement time
MI	2	Cards in pre-arranged order
MII	4	
MIII	8	
MIV	13	

Movement times. This part of the experiment attempts to estimate the movement time. Hence a subject is instructed to carry out the procedure as nearly as possible with the same movements as those employed by him in the previous tasks. He is told how in Tasks MI, MII, MIII and MIV (see Table XV) the cards have been prearranged to eliminate all choice.

The same timing and scoring procedure is followed as for the balanced frequency tasks.

Unbalanced frequencies. So far the number of cards in each class for any particular task has been equal. For this part of the experiment it is explained to a subject how the numbers are deliberately unbalanced by using the frequencies given for Tasks UBI, UBII and UBIII (see Table XV). He again sorts the cards whilst the experimenter records the total times as with the other procedures.

Since it is possible for a subject to count the number of cards in the smaller class and to speed up his operation when all the members of that class have been sorted, it is advisable that one member of a small class be inserted amongst the last 2 or 3 cards in the pack. Thorough shuffling of the pack is important at this stage.

Treatment of results. When the probabilities of all the different classes of signals are equal, the average information per signal is the same as the information in any one class. Thus $H = -\log_2 p = -\log_2 \frac{1}{n} = \log_2 n$ and the appropriate value can be read straight off the $\log_2$ table (see Table XVI). For example, when sorting into suits (BII), each class has a probability $p = 1/4$, and therefore the information carried by any one card is $H = -\log_2 1/4 = 2$ bits.

When the probabilities of each class are not equal, the average

TABLE XVI. Logarithms to base 2
(Numbers from 1 to 99)

n	0	1	2	3	4	5	6	7	8	9
0	—	0·000	1·000	1·585	2·000	2·322	2·585	2·807	3·000	3·170
1	3·322	3·459	3·585	3·700	3·807	3·907	4·000	4·087	4·170	4·248
2	4·322	4·392	4·459	4·524	4·585	4·533	4·700	4·755	4·807	4·858
3	4·907	4·954	5·000	5·044	5·087	5·129	5·170	5·209	5·248	5·285
4	5·322	5·376	5·392	5·426	5·459	5·492	5·524	5·555	5·588	5·615
5	5·644	5·672	5·700	5·728	5·755	5·781	5·807	5·833	5·858	5·883
6	5·907	5·930	5·954	5·977	6·000	6·022	6·044	6·066	6·087	6·108
7	6·129	6·150	6·170	6·190	6·209	6·229	6·248	6·267	6·285	6·304
8	6·322	6·340	6·358	6·375	6·392	6·409	6·426	6·443	6·459	6·476
9	6·492	6·508	6·524	6·539	6·555	6·570	6·588	6·600	6·615	6·629

information content per signal is reduced. Its value is calculated as follows: Let us consider task UBI, i.e. the sorting of picture cards from plain cards. Here the signal 'picture card' has a probability $p = 40/52 = \cdot 77$. For the class 'picture cards' the information content is $-\log_2 12/52 = 5\cdot 700 - 3\cdot 585 = 2\cdot 115$ bits. Similarly, for the class 'plain cards' the information content is $-\log_2 40/52 = 5\cdot 700 - 5\cdot 322 = \cdot 378$ bits. On the average then, over all signals in a series of 52 cards, the information content per signal is $H = -p \log_2 p = (\cdot 23 \times 2\cdot 115) + (\cdot 77 \times \cdot 378) = \cdot 77301$ bits (which is some 20 per cent less than in a two-choice balanced frequencies case, e.g. 'red-from-black', where $p = \cdot 5$ for each class and $H = 1$ bit per card).

In attempting to estimate the rate of gain of information we must bear in mind that in our experiment the subject himself determines the time when each signal shall occur, so that Hick's suggestion of writing $(n + 1)$ for n does not apply.

We calculate thus (hypothetical results):

		Average values of:				
Task	*Number of Choices*	*Information per signal*	*Response time*	*Movement time*	*Choice time*	*Rate of gain of information*
	n	$H = \log_2 n$ bits/card	T_r sec/card	T_m sec/card	$T_c = T_r - T_m$ sec/card	$R = \frac{H}{T_c}$ bits/sec
BI	2	1·00	e.g. ·87	·58	·29	3·45
BII	4	2·00	e.g. 1·19	·60	·59	3·38
BIII	13	3·70	e.g. 1·71	·66	1·05	3·52
		$H = -\Sigma p \log_2 p$ bits/card				
UBI	2	·77	e.g. ·83	·58	·25	3·08
UBII	4	1·76	e.g. 1·15	·60	·55	3·20
UBIII	8	2·77	e.g. 1·42	·64	·78	3·55

It should be found that, since response latency is a function of information transmitted, the subject's rate of gain remains, to a good approximation, constant.

II. PERCEPTUAL FACTORS IN MOTOR PERFORMANCE

In the card sorting experiment we examined the way in which the degree of uncertainty about the occurrence of a particular signal determines the time to make an appropriate choice. In this section we shall be concerned with further perceptual problems. We shall begin by examining how differences between signals may influence performance and we shall conclude by considering the role of perceptual factors in a typical motor assembly task.

When we consider the differences between signals it would seem reasonable to expect that it would be easier to sort black cards from red than, say, hearts from diamonds, though each would be a two choice task. Both Henmon (1906) and Lemmon (1927) carried out experiments to assess the perceptual difficulty in a display. The latter presented two square frames to his subjects who had to respond to the side which contained the most illuminated bulbs. A subject had to discriminate 1 from 0, 2 from 1, 3 from 2 and so on. As might be expected the simple reaction of 200 ms rose sharply to 290 ms when discriminating 1 from 0 lights, 475 ms for 2 from 1, and 741 ms for 5 from 4 lights.

Perceptual discrimination in card sorting. Following Crossman (1955) this experiment can again be conducted with card sorting. Plain cards are used on which are drawn varying numbers of small circles, say 1 to 12. These are positioned randomly over the face of each card. The object of the experiment will be to relate the time required by a subject to sort out a pack of 52 cards to the degree of similarity between the patterns of circles, which are being varied, whilst the number of alternatives from which a response has to be chosen is kept constant. For a given condition two sets of cards, e.g. two's and eight's, are shuffled well together and given to a subject face downwards. Appropriate sample cards are laid out in front of him and he is asked to sort the cards into two heaps, turning the cards up one by one and deciding between 'fewer' and 'more' circles without actually counting them. The total time is recorded as before by stop-watch.

We may first consider the situation when there are equal absolute differences between the number of circles in each pair of classes. For example we may obtain times for blank compared to 1, 1/2, 2/3 . . ., or blank/2, 2/4, 4/6 . . ., or blank/3, 1/4, 2/5 . . . etc. To estimate movement time we repeat the performance as nearly as possible with a prearranged pack, say 1-2-1-2 . . . The effects of practice can be partly eliminated by presenting the conditions in random order, three or more trials for each condition as a practice run, and then in another order for the experimental run. Conditions in which cards with many circles have to be employed are likely to require more practice before a subject settles down to a steady rate.

We may next consider the effect of equal ratios between the numerical size of circles in each pair of categories by obtaining times for the 1/2, 2/4, 3/6, 4/8 and 6/12 conditions.

Two features emerge from the results:

(*a*) Time increases with the number of circles on a card, although absolute difference within each pair of categories remains constant.

(*b*) To a good approximation, equal ratios give identical sorting times.

This dependence on ratio suggests that when two quantities, say A and B, are being compared by a subject in a discrimination task, his response time is a simple function of the difference between the logarithms of the quantities involved in the comparison. Crossman gives the formula

$$\mathrm{RT} = \frac{\mathrm{K}}{\log \mathrm{A} - \log \mathrm{B}}$$

(where K is an arbitrary constant) as fitting the data adequately. The results of the experiment can be plotted against this function (the values of which are given in Table XVII). If the theory is correct we should obtain an approximately straight line whose slope is determined by K. Other points, e.g. 2/5, 3/7, etc., can now be extrapolated and the sorting times predicted.

Perceptual factors in weight discriminations. We may now attempt to ascertain whether this way of measuring the perceptual difficulty of a 'display' would apply to a sensory modality other than vision. Let us consider a modification of the classical experiment on the differential threshold for judgement of weight. Since in this case we are not interested in thresholds, we shall work with relatively easy

TABLE XVII. Values of function $\dfrac{K}{\log A - \log B}$

(Numbers from 1 to 10)

x_2/x_1	1	2	3	4	5	6	7	8	9	10
1	φ	1·00	·63	·50	·43	·39	·36	·33	·31	·30
2		φ	1·71	1·00	·76	·63	·55	·50	·46	·43
3			φ	2·41	1·36	1·00	·82	·71	·63	·58
4				φ	3·13	1·71	1·25	1·00	·86	·76
5					φ	3·82	2·07	1·48	1·18	1·00
6						φ	4·51	2·41	1·71	1·36
7							φ	5·18	2·76	1·94
8								φ	5·88	3·13
9									φ	6·58

judgements only, and instead of the proportion of correct judgements, the response time will be taken as a measure of performance. Essentially the same technique is employed as in the card sorting experiments. For instance, two groups of 10 small cans, loaded with two different weights, can be arranged in random order to form a row on a table, facing the subject. At the word 'go' the subject lifts first a sample 'light' and a sample 'heavy' can, then works down the row, weighing very briefly each can and moving 'heavy' cans away from him and 'light' ones towards him. As before, a subject should work as fast as possible, without making errors, prolonged weighing being discouraged. The time for the whole row is taken on a stop-watch. The pairs of weights may be arranged in the following ratios: 0/8, 2/8, 4/8 and 6/8, the order of presentation being random. About half a dozen trials in each condition may be required to obtain fairly consistent times, with perhaps one or two errors being made occasionally. Again movement time may be assessed by using any pre-arranged task (e.g. 2-8-2-8 . . .), the subject knowing that this is so, but endeavouring to employ the same movements as in the main tasks. The results are plotted against the appropriate values of the theoretical function and compared with the visual tasks.

EXPERIMENT II. PERCEPTUAL FACTORS IN THE CONTROL OF MOVEMENTS

In the previous reaction time studies we measured how long a subject takes to initiate a movement. In this experiment we wish to measure how long he takes to make a particular movement. Experiments have shown that in general the two most important variables influencing these movements are the amplitude and the permitted tolerance limits. In some specific cases manipulatory acts such as finger adjustment to grasp or release objects, and the pressure required to carry an object with safety but without damaging it, have also to be considered. But for many assembly types of task we can say that where subjects are working at their maximum rate their average response time is proportional to the variations in the amplitude and tolerance conditions. The following experiment examines this hypothesis.

The apparatus requirements are a pencil and some large sheets of paper. On each sheet of paper are drawn two circles at 2, 4, 8 or 16 inches apart. The diameters of the circles are ·125, ·25, ·50, and 1·0 inch (see Table XVIII). A subject is instructed to place his pencil in the left hand circle and then on the command 'go' to move his pencil as rapidly as possible to make a point in the right hand circle. He is then to return and repeat the action in the first circle, and continue the to and fro movements until he has made 20 shots at each circle. He is to respond as quickly as is compatible with accuracy. The experimenter times the procedure with a stop-watch, counting the number of crossings, and tells the subject when to stop at the end of a trial. A subject is tested twice at all 16 conditions, the order of conditions being reversed on the second testing.

Where the distance between the circles is small and the tolerance large, the whole cycle is extremely rapid and it is not easy to register accurately the number of responses, though it can be achieved. Where it is possible to conduct the experiment on a moving paper band, driven by a constant speed motor, the record can be accurately measured in terms of the distance travelled by the paper. With this apparatus the tolerance at each end is given, not by circles, but by two parallel lines which are the same distance apart as the diameters of the circles.

Treatment of results. When the results are grouped, it will be found that the times increase as the amplitude of the movement is increased and as the tolerance of the target areas is reduced.

The query now arises whether the two variables cannot be combined so that the difficulty of the task is expressed in their terms. Fitts (1954) has proposed one solution. He accepted Hick's (1952) original suggestion that the rate of gain of information is constant with respect to time. The argument is that the human motor system (in which is included the visual and proprioceptive feedback loops that enable a subject to monitor his own activity) has a fixed information-transmission capacity. It would follow that where subjects were working at their maximum rate, then the average time per response would be directly proportional to the maximum average amount of information per response. In an experiment of this kind the information per response is controlled by the two variables manipulated by the experimenter, the amplitude and the tolerance conditions. Where such repetitive movements are speeded up then each movement will provide less information with the result that movement variability will increase by a specified amount. Fitts' proposal, in its simplest form, may be stated as follows: that the overall movement time will be proportional to the logarithm of the rates between (*a*) the width of the targets (Ws) and (*b*) twice the amplitude of the movements (2A). Expressed as a formula the difficulty of the task (Id) is:

$$\mathrm{Id} = -\log_2 \frac{\mathrm{Ws}}{2\mathrm{A}} \text{ bits/response}$$

where Ws is the difference in inches between the diameter of the target area and the diameter of the object being positioned, and A (the amplitude) is the centre-to-centre distance between the holes. The value for the index of difficulty is given for each of the tasks in Table XVIII.

In a task such as the present where the pencil point can be taken as zero width, we simply measure the diameter of the target area to obtain Ws. In order to see how adequately the suggestion of a fixed transmission capacity accords with the experimental data, we need to examine the difficulty of the task against the time taken to perform it. If the concept is useful, then there should be a clear relationship between the two measures. This can be studied most conveniently by calculating a binary index of performance (Ip) on the basis of

TABLE XVIII. Experimental conditions for positioning task

Diameter of circle in inches (Ws)	*Amplitude of movements in inches* (A)	*Index of difficulty* (Id)
·125	2	5
·125	4	6
·125	8	7
·125	16	8
·25	2	4
·25	4	5
·25	8	6
·25	16	7
·50	2	3
·50	4	4
·50	8	5
·50	16	6
1·00	2	2
1·00	4	3
1·00	8	4
1·00	16	5

Id is discussed in the text. It is derived from the formula

$$\mathrm{Id} = -\log_2 \frac{\mathrm{Ws}}{2\mathrm{A}} \text{ bits/response}$$

dividing the index of difficulty (Id) by the average time (t) in seconds for *a single movement.*

$$\mathrm{Ip} = \frac{\mathrm{Id}}{t} \text{ bits/sec}$$

Taking the experimentally obtained 't' values for each condition of the task, this rate of performance is calculated and, providing subjects have worked at their maximum, should be approximately the same for all task conditions. With unpractised subjects it is usual to find some variation, but the greater the practice the more consistent the index of performance.

It was appreciated by Fitts and shown by subsequent work (Annett, Golby and Kay 1958) that there are some arbitrary assumptions in the above formulae. It would not always be rational to take twice the amplitude of the movement, as in the case of a long arm movement where 2A might be a physical impossibility, whilst the tolerance depends less upon a circular area as upon an ellipse whose longer axis is a prolongation of the direction of the movement. But these are minor points for future clarification. The significance of the work lies in its approach which places the emphasis upon the perceptual factors controlling motor skills. To this extent these studies of movements are closely related to those which we have already examined on reaction times. It would appear that the same major variables are influencing motor performances which superficially appear to be very dissimilar.

III. DISPLAY-CONTROL RELATIONSHIPS

High information-transmission rates depend, among other things, upon the degree to which the relation between items of display and characteristics of control is regarded by the subject as 'natural'. As might be expected, the difficulty is to understand what is meant by 'natural'. Anything which an operator has to learn may be considered initially by him as unnatural, whilst many accepted relationships are only the result of long practice: for example, pressing a switch in the corner of a room to operate the centre light. In this section we shall attempt to inquire in a preliminary manner what perceptual cues are desirable when translating visual signals into motor responses.

A popular experimental procedure is to study how display-control relationships are initially built up. This is generally examined by reversing a habitual relationship as in a mirror-drawing task. When an individual attempts to control hand movements by observing them in a mirror he makes many mistakes. These can be scored but a simpler procedure is to ask a subject to be as fast and accurate as possible and score the time taken to carry out the task. This is legitimate since most errors inevitably lengthen the performance time.

A representative experiment (task Ma) is to require a subject to move a pencil or stylus through a narrow groove, pressed in tinplate or cut out in wood. The 'maze' is placed horizontally in front of a subject but a horizontal screen, 6–9 inches over the maze, prevents him seeing it directly and his only vision is via a vertical mirror positioned at the far end of the maze. The track forms a wavy pattern with 12 reversals of different amplitude. Typical instructions are to tell a subject to drive a car along the 'road' as fast as possible without hitting the edges. Retracing is discouraged. Some 6 to 12 trials are completed with short rest pauses between each, the time for each trial being taken by stop-watch.

A second experiment (task Mb), which superficially appears to be the same, is to invite a subject to move a pencil in a similar manner between two lines, drawn a short distance apart to form a wavy pattern on a sheet of paper and again seen only via a mirror. Times are recorded as above.

Finally, task Mc, a subject has to write down on a sheet of paper the digits 1 2 3 4 5 6 7 8 9 0, so that their normal – i.e. unreversed – images appear in the mirror. Times are again scored.

It is found that most subjects can, after practice, master quite adequately the perceptual relations in these tasks, provided both visual and kinaesthetic guiding patterns are available (as in task Ma). Some subjects may encounter a little difficulty in perceptual relearning when performance has to be guided primarily by visual pattern as in task Mb, but they should improve with practice. However, many more subjects are likely to remain at a disadvantage when no distinct pattern for the movements is provided and the imagined display items have to be held in mind during the course of the performance as in task Mc. The argument might be raised that in this particular case the display items constitute very familiar material and the task as a whole refers to a well established habit – writing figures – so that perceptual relearning is rendered more difficult than in the other two tasks. But this criticism applies to all three tasks. They are not so much studies of how subjects learn to co-ordinate the signals from different sense modalities as studies of how subjects learn to reverse the meaning of particular signals. They involve the breaking down of well established motor co-ordinations. This is not to say they are valueless but we must be cautious in interpreting results. In general, individuals have as much, if not more, difficulty in reversing habits than in learning them.

We may pursue the question of 'matching' of visual and kinaesthetic data in repetitive tasks of the kind here considered by inquiring with what accuracy can a subject move his hand to a specified point and then return it to the initial position when visual information is withheld during the performance. The subject, blindfolded and seated at a table, is required to raise slightly his dominant forearm, holding a pencil, and to carry out a smooth movement at what appears to him a comfortable speed. The starting point is in front of the midline of the body, the terminal position being a screen at a distance of 12–15 inches to the side; this particular distance is roughly midway between the extremes of extension and flexion, and can therefore be assumed to minimize their cues. An alternative method is to place the screen at the midline and move towards the centre. The subject is invited to carry out a dozen or more such movements under each of the two following conditions:

(*a*) NKR – no knowledge of results; after the completion of each movement the experimenter guides the subject's hand to the appropriate starting point;

(*b*) KR – with visual knowledge of results; after the completion of each movement the subject is allowed to open his eyes (or to lift dark goggles, if used), to note the deviation and to return his hand to the starting position. For obvious reasons the first movement in this condition is not scored.

The records are examined for the frequency of over- and under-correction, as well as for the extent to which the subject benefits from the visual knowledge of the result of each blindfold movement. Clearly in condition KR his performance involves 'translating' a visual cue into a motor response. However, this description might seem inadequate in that it does not allow for the obvious importance of how well a subject is remembering the information which he receives. If he does not retain it accurately then subsequent errors may be due to memory losses rather than to inaccuracies of translation. It seems, therefore, desirable to proceed from this brief exploration of blindfold performance to a more detailed study of eye-hand co-ordination under conditions when visual information from the display and kinaesthetic information from the limb are allowed continuously, but vision of the limb is excluded.

EXPERIMENT III. 'MATCHING' OF VISUAL AND KINAESTHETIC DATA

Many skilled actions require that the hand, itself unseen, should be moved to a position dependent on that of some other item in space which can be seen. We need, therefore, a task which would demand that the limb, itself unseen, be moved over a distance specified by the size of an agreed misalignment – the latter being available for inspection throughout the execution of the movement, so that a subject does not have to rely upon his memory. With these points in mind, it is arranged that a subject is seated at a table which has a second horizontal surface placed six inches over the normal surface. The top level carries a display of small disks numbered 1 to 10, arranged at varying distances of 3 to 18 inches from an additional disc marked X at the edge in front of the subject's midline. The lower level has a large drawing pin, corresponding exactly to the position of the X disc, and represents the starting point. This surface is covered by a large sheet of paper or cardboard. It is arranged that the subject can see neither the lower level of the table nor his arm (a lightweight cloth may be used for the purpose). He is invited to move his hand, holding a pencil, from the starting point and over the distance indicated by the position of the numbered disc, to write down this number and then to return to the initial position. In this manner a subject deals with the successive discs. When all the moves have been completed, the experimenter records the individual errors. Two or three sets of readings are taken.

In the second part of the experiment a subject carries out another set of movements when the display of misalignments is presented at the same distance from the eyes, as measured from the centre of the array of discs, but with the display now in the vertical plane. Again the subject's own movements are screened from his vision. Two or three complete sets of readings are taken, the experimenter recording the individual errors at the end of each set.

Discussion. It will be found, not surprisingly, that under the first – the horizontal – conditions, a subject will tend to move his hand to positions which 'feel' to be directly underneath the points indicated on the display. It is not by any means clear how this kind of 'projection' is in fact achieved, though there is a residual error of some 10 to

20 per cent, and perfect hits may not be observed. But considerably larger errors are made when the misalignments are presented vertically. Here a subject has to observe visually the spatial distance between the points and then to translate this distance into a movement in which the only feedback information is kinaesthetic. It may be argued that the task could be carried out by ballistic movements, that is, a subject triggers off a movement of a particular force and extent, such movement not being controlled by any feedback information. But most subjects do not make such responses in this experiment. They tend to move relatively slowly and are obviously dependent upon kinaesthetic signals to indicate the position of the moving arm. The horizontal condition measures how accurately a subject can position his limb using only kinaesthetic information. The vertical condition measures how accurately he can do this when he has first to imagine where the vertically observed distance would fall in the horizontal field and then carry out a visual – kinaesthetic 'translation' of that distance.

As yet little is known about how individuals are able to carry out such motor responses, though these 'translation' processes are a common feature of many skills. When a particular movement is initiated, the musculature can certainly signal feedback information about how far the response has or has not been executed. But it has to be recognized that many skills first demand some symbolic translation from an arbitrary criterion, such as producing a movement of 12 inches, or making a semicircular trajectory, or applying a particular pressure. The evidence so far suggests that it takes a measurable time to carry out these translations, for the closer the correspondence between signal and response the quicker the initiation. It is now being suggested that with very high stimulus-response compatibility the multi-choice reaction times as found in Merkel's and Hick's experiments can be reduced. If in one situation the light-stimuli and the key-responses are both arranged horizontally and in close one-to-one correspondence it is found that the choice reaction time is shorter than in the situation where the lights and keys are not spatially adjacent or where the signals symbolically indicate the appropriate responses. There is no doubt that a high correspondence between signal and response is a prerequisite if a skill is to be acquired quickly.

CONCLUSION AND SUMMARY

This chapter has tried to bring out the underlying relationships between a number of motor responses which at first inspection appear dissimilar. The approach has been to look not so much at the musculature but at what is controlling it, less at the responses and more at the signals which have to be received before such responses can be made. The problem has in some ways been inverted with the emphasis being put upon what is happening upon the perceptual side rather than upon the motor. It is recognized that there is a deficiency in our studies of the motor output *per se* but the emphasis upon the perceptual side has produced results and leads to a greater understanding of the problem. For a long time, reaction time studies were being discussed as if they were only of historical interest. Now it is clear that this is by no means so. The reaction time results bring out the basic limitations of the human system in responding to discrete signals. It is because the human organism does react to stimuli with a definitely measurable time delay that we recognize the skill in anticipatory responses. It is through understanding how reaction times decrease as the probability of the signal increases that we have a most important clue to understanding what happens as skills are learned. The trained operator knows the interdependence of signals – given 'a' that 'b' will follow – and as a result he can handle many more signals, though not more information, in a particular unit of time.

We may summarize the position as follows:

1. The behaviour of a human subject performing a motor task is viewed as a process in time beginning with the abstraction of information from a variety of sensory cues about changes in the external world, and giving rise to some form of motor activity, which in turn influences the reception of further information available through the senses.

2. Intervening between the receptor and effector functions are the analytical operations of the brain, the 'decision' and 'translation' which are concerned with selecting an appropriate action from a 'repertoire' of possible actions. The time for such decisions is markedly influenced by the relationship between the display (signals) and the response – what is now often called the stimulus-response compatibilities.

3. A further factor influencing the time duration of these perceptual processes is the probability of a signal, the greater its uncertainty, the longer the time to receive it. So much learning is an effort to reduce this uncertainty, thereby allowing the operator to predict future signals and begin preparing his responses.

4. Without the ability to predict signals, the characteristic 'smoothness' of skilled sequences would not be possible. Decisions have to be made in 'anticipation' of the next signal whilst responding to the previous item in a series.

REFERENCES

ANNETT, J., GOLBY, C. W., and KAY, H. (1958). 'The measurements of elements in an assembly task – the information output of the human motor system'. *Quart. J. exp. Psychol.*, **10**, 1.

BARTLETT, SIR FREDERIC (1947). 'The measurement of human skill' (Oliver Sharpey Lectures). *Brit. Med. J.*, **1**, 835.

BREITWIESER, J. V., (1911). 'Attention and movement in reaction time'. *Arch. Psychol.*, **2**, 18.

BRYAN, W. L., and HARTER, N. (1897). 'Studies in the physiology and psychology of telegraphic language'. *Psychol. Rev.*, **4**, 27.

BRYAN, W. L., and HARTER, N. (1899). 'Studies in the telegraphic language'. *Psychol. Rev.*, **6**, 345.

CROSSMAN, E. R. F. W. (1953). 'Entropy and choice time: the effect of frequency unbalance on choice-response'. *Quart. J. exp. Psychol.*, **5**, 41.

CROSSMAN, E. R. F. W. (1955). 'The measurement of discriminability'. *Quart. J. exp. Psychol.*, **7**, 176.

FITTS, P. M. (1954). 'The information capacity of the human motor system in controlling the amplitude of movement'. *J. exp. Psychol.*, **47**, 381.

HENMON, V. A. C. (1906). 'The time of perception as a measure of differences in sensation'. *Arch. Philos. Psychol. Sci. Meth. N.Y.*, **8.**

HICK, W. E. (1952). 'On the rate of gain of information'. *Quart. J. exp. Psychol.*, **4**, 11.

LEMMON, V. W. (1927). 'The relation of reaction time to measures of intelligence, memory and learning'. *Arch. Psychol. N.Y.*, **94.**

MERKEL, J. (1885). 'Die zeitliche Verhältnisse der Willensthätigkeit'. *Philos. St.*, **2**, 73.

PIÉRON, H. (1928). 'Technique de laboratoire et appareils'. *Ann. Psychol.*, **27**, 23[illegible].

WOODROW, H. (1914). 'The measurement of attention'. *Psychol. Monog.*, **17**, 158.

WOODWORTH, R. S., and SCHLOSBERG, H. (1954). *Experimental Psychology*. London: Methuen.

The following references which have appeared subsequent to the writing of this chapter would be found particularly useful:

LEONARD, J. A. (1959). 'Tactual choice reactions: I'. *Quart. J. exp. Psychol.*, **11**, 76–83.

MOWBRAY, G. H., and RHOADES, M. V. (1959). 'On the reduction of choice-reaction times with practice'. *Quart. J. exp. Psychol.*, **11**, 16–23.

WELFORD, A. T. (1960). 'The measurement of sensory-motor performance: survey and reappraisal of twelve years' progress'. *Ergonomics III*, 189–230.

Chapter 6

THINKING

B. M. FOSS

Birkbeck College, London University

INTRODUCTION

It is convenient to consider thinking in two ways. A thinking man can describe his thought processes in terms of the ideas, images and perhaps feelings which occur to him. These can be called the content of his thought processes, and are usually investigated by introspection. Alternatively, thinking can be described in terms of its dynamics, for instance its direction, continuity or lack of continuity, and speed. These can be studied by behaviourist techniques (as will be seen later in this chapter) as well as by introspection.

The content of thinking. Experiments on the content of thinking are possible, but their results are often difficult to interpret. It is possible, for instance, to present a subject with a problem to solve, and to ask him to introspect after solving it. Similarly he may be asked to report on his thinking while in a drugged condition, or in other states of altered consciousness. His reports may provide a wealth of detail on the ideas and images which passed through his mind, but such reports are of doubtful scientific value for three reasons: first, they suffer from the deficiencies of all subjective reports; secondly, the individual differences in the content of thought (the Würzburg group, *passim*, and McKellar 1957) are so great that the results from such experiments tend to be specific to the person; thirdly, as will be seen in the next paragraph, it is often difficult to trace any relation between what a man is thinking and where his thoughts are taking him. That is, the content and dynamics of thinking are not clearly related. It is as though a man's thoughts were often irrelevant to his behaviour.

For these reasons, experiments on the content of thought do not often find a place in the teaching of experimental psychology. The exceptions are provided by experiments similar to those of the Würzburg school (Humphrey 1951). In the work of this school are to be found some of the most important experiments using introspection; and paradoxically the overall results underline the inadequacy of introspective method. The climate of thought up to the time of these experiments was that there could be no thought without conscious content; that this content was made up of sensations, images or feelings; and that the progression of thoughts was determined by previously formed associations between the items making up this content. The Würzburgers asked each other questions of the kind 'What are the colours of the windows in the Sistine chapel?' or 'One must be both pitiful and cruel in order really to be either. True or false?' and as a result of introspective reports they concluded that the conscious content of thought was often 'empty', and the direction of thinking could not be explained solely in terms of associations within the content, but depended also on the task which the subject was tackling. (Occasionally the underlying process does come to mind if the task is interrupted.) It follows that introspective reports are inadequate in describing the direction of thinking, both because of the gaps in thought content, and because there is an underlying directing process which itself is not accessible to introspection. The first experiment of this chapter illustrates such an underlying process. These processes are sometimes spoken of as being unconscious, and before describing the experiment it is necessary to say something about unconscious processes, and also about the concepts of 'gap-filling' and 'lack of closure'.

Unconscious processes. These are thought of mainly in the context of depth psychology, such as that of Freud. However, such processes have been invoked for many years to explain phenomena in other realms of experience, for instance by Helmholtz in his use of 'unconscious inference' in perception, and many examples have been collected of unconscious processes in thinking (Carpenter 1874, chap. 13; Hadamard 1945). An analysis of these examples shows that explanations in terms of unconscious processes are given whenever something occurs in thinking which does not appear to follow directly from what was being thought a moment before. Hadamard gives many instances of mathematicians thinking about a problem, then leaving it to 'incubate', when finally a solution may occur suddenly – as though the problem had been solved unconsciously. Also the gestalt psychologists have shown that sudden insight is common in problem solving, while other psychologists and some philosophers talk of the 'aha experience'. There are few experiments on the factors which give rise to these phenomena, although it is possible to demonstrate these things happening. Problems of the kind: 'How would you construct four equilateral triangles from six matches?' and 'Which is worth more, half a ton of sovereigns or a ton of half-sovereigns?' often lead to a wrong answer, followed by a 'flash of insight' and a correct answer. It may be that underlying the insight is a sudden change in viewpoint, rather than any unconscious process, and these 'trick' problems are often framed in such a way that the solver is forced into a wrong approach to begin with. For instance, match problems are usually solved in two dimensions, whereas the one given here needs a solution in three dimensions, by constructing a pyramid; and the second problem is rather like $\frac{1}{2} \times 1 = 1 \times \frac{1}{2}$.

In these cases, then, the set is induced by the form in which the problem is given. In other cases (Maier 1930) it has been shown that a set can be induced by an object or incident in the immediate environment. For instance, a swinging rope may suggest the construction of a pendulum in order to solve a problem, even though the subject is not aware afterwards that he saw the rope swing.

Yet a third source of set is 'lack of closure'. *Lack of closure* is a concept used by Gestalt psychologists. In the principle of *Prägnanz* they claimed that an unclosed percept will tend to close, because the concomitant physiological processes will tend to close. For instance, a circle with a small gap in it, if seen for a short enough time, will be seen as a closed circle. Lack of closure, they said, leads to tensions requiring resolution; and by translating this concept from spatial closure to temporal closure, they arrived at the idea that an incompleted task results in similar tensions. Thus, Ovsiankina (1928) showed that if children were interrupted in the course of a task, they then showed a marked inclination to resume the task later, as though to reduce the tension. Lack of closure is seen as a directing and motivating source.

Indeed the Gestalt psychologists sometimes described the process of thinking itself as *gap-filling* and other psychologists (Selz, see Humphrey, chap. 5; Bartlett 1958) have used similar descriptions. In Bartlett's case, thinking is seen as interpolating or extrapolating from the data of a problem, and so filling a gap between the data and the desired goal.

The following experiment demonstrates that the direction of thinking may be influenced by an incompleted task without the subject's knowledge. Perhaps it also illustrates a phenomenon reminiscent of the incubation said to occur in unconscious thought processes.

EXPERIMENT I. UNCONSCIOUS FACTORS IN THINKING

First experiment. This requires more than one subject, and the subjects must not be aware of the purpose of the experiment. It is therefore not typical of the experiments presented in this book. It is followed by an alternative experiment on unconscious processes which can be performed on a single sophisticated subject.

The experiment consists in giving subjects a series of scrambled words to unscramble within a given time interval. The time interval is chosen so that the S fails to solve some of the words. Later S is given a second set of problems which has no *prima facie* connection with the first. These problems consist of simple crossword puzzle type clues, each of which has two possible solutions. For each clue one of the solutions is also the correct solution, which S failed to get, of a scrambled word presented previously. The hypothesis to be tested is that this solution will be chosen rather than the alternative.

Material and considerations arising from it. The clues in the second part of the experiment can be of the kind:

Part of each year - U - U - -

There are two solutions of this: AUTUMN and AUGUST. It is possible that these are not equally probable solutions. Supposing the experiment is designed to 'force' S to choose AUTUMN, then in the first part of the experiment S will have been given, say, AMUNUT to unscramble; if he failed to solve it, he will, in the second part of the experiment, have been given the above clue. If, to this clue, he provides the answer AUTUMN, one cannot be certain whether this is due to the experimenter's manipulation, or an existing preference for autumn rather than August, or to any other factors. The 'any other factors' can be controlled to some extent by giving the experiment to many subjects when, on the average, these factors should cancel each other out. It would also be possible to find out in a preliminary experiment the relative frequency of choice of AUTUMN and AUGUST within the population from which the subjects are to be drawn for the main experiment. As it happens, this is unnecessary, since this factor can be controlled in a simple way by giving half the Ss AUTUMN in scrambled form and half AUGUST in scrambled form. The prediction is then that fewer in the second lot of Ss will choose AUTUMN than will in the first lot of Ss.

The material will therefore consist of several clues of the kind quoted above, each of which has two more or less equally probable solutions. These clues should be printed out on separate slips of paper, with the number of letters indicated by dashes, and those letters which are common to both solutions inserted to help the solver. Other examples are:

Associated with soldiers	B - LLET	(billet, bullet)
A shopkeeper	- R - - ER	(draper, grocer)
Listened to in church	- - R - ON	(parson, sermon)

Each of the possible solutions in scrambled form should also be printed on separate slips of paper. There is no obvious reason for adopting any uniform plan in scrambling the words. For convenience in running the experiment it is important that they should not be too easy to unscramble. It may be that E should avoid words which can be unscrambled in more than one way, but there is no experimental evidence on this point. The question of the number of problems to set is discussed below. Some additional, trial, scrambled words should also be prepared.

Method. Ss should be instructed that they are to be asked to unscramble some words within a time limit (not stated). Two or three trial words are then given and the time to solve each in turn is taken. This is done to give E some idea of the speed of S, and also to familiarize S with the task. Without a break, the experimental words are then given one at a time. In each case, if possible, S should be interrupted ('time up') before solving the word, but not before he has had time to get involved in the problem. Half the Ss should, of course, be given the scrambled version of one solution to each clue, the other half being given the scrambled version of the alternative solution.

After an interval of one minute, which should be occupied by conversation (to prevent S thinking further about the scrambled words and possibly solving them), the second part of the experiment can be started. (It is important that S should not realize that there is any connection with the first part, and some ingenuity may be necessary to achieve this.) The prepared clues are then given to S, one after another, and he is asked to solve them as rapidly as possible, but he is in fact allowed as much time as is necessary to solve them. Here again, a couple of trial clues may be used before the experimental ones, to familiarize S with the task. If, at any stage, S realizes how the two parts of the experiment are related, the experiment on that S should be abandoned, though any results obtained up to that point may be used. (It is most important that S should not be given the clue corresponding to a word which he successfully unscrambled, since this would lead to S's seeing the connection between the two parts of the experiment.)

Treatment of results. The results from all subjects for *any one clue* can be combined in a 2 × 2 frequency table. For instance, in the case of AUTUMN versus AUGUST:

	Presented with scrambled version of:	
	AUGUST	AUTUMN
Number choosing AUGUST	*a*	*b*
Number choosing AUTUMN	*c*	*d*

If the frequencies *a* and *d* are large compared with *b* and *c*, the results indicate a tendency in the direction hypothesized. If the number of Ss has been sufficiently large, the significance of these results can be assessed by the chi-square test. Such a test would have to be applied separately for each pair of words.

Another treatment is as follows: if the subjects were divided exactly in half when given the alternative scrambled words, that is if $a + c = b + d$, then all the results can be lumped together as follows:

Number of occasions on which the 'forced' word was chosen ($= x$).

Number of occasions on which the alternative was chosen ($= y$).

By chance, x should equal y on the average. Departures from this average can be tested by chi-square. If x is significantly greater than y, the hypothesis is supported.

Number of subjects. If the first treatment is to be used, in which the results for each pair of words are analysed separately, then something like twenty subjects will be necessary. With the second treatment, number of subjects multiplied by number of pairs of words used should be ten or greater to test significance. Unless the pairs of words have been carefully chosen so that they occur equally often as answers to the clue, this product, pairs of words × subjects, should be a good deal larger. Five pairs of words given to four subjects might be considered minimal.

Number of word pairs. The same considerations hold. If twenty or more subjects are available, only one or two word pairs need be used. At the other extreme, if the number of scrambled words which a subject is *not* allowed to solve is very large, the 'load' of incompleted tasks may be too great for them severally to affect later tasks.

Possible uses of the experimental technique. Several variants and extensions of this experiment are possible. The last paragraph suggests an investigation of the interaction, if any, of incompleted tasks on each other. Is there a limit to the number of incompleted tasks which may effectively direct behaviour? Are there individual differences between people in such limits? Is there any phenomenon resembling retroactive interference or inhibition which operates on 'sets' acquired by not being allowed to complete tasks?

Perhaps a more useful field to explore might be: What is the optimal time interval between the first and second part of the experiment? What are the limits of this time interval for any effect to show at all? What are the effects of varying conditions during the time interval (e.g. sleep; complete change of task) and during the second part of the experiment (fatigue; excessive incentive)?

Finally, there may be other pursuits beside solving scrambled words which may be interrupted to generate a specific set; and there may be tests which are more sensitive than crossword puzzle clues for tapping that set.

Second experiment. Both Messer and Watt (Humphrey 1951) used what has been called *constrained association* as a method of investigating the determining tendencies or *Aufgabe* underlying 'associative' thinking. In this method, the Subject is given a stimulus word to which he must respond with an associated word, but the response word cannot be any associate; it must have a certain relationship to the stimulus word. For instance, if the subject is instructed that he must respond with a co-ordinate, the stimulus 'dog' might lead to the response 'cat'; that is, another member of the class. If the instruction was for a *superordinate*, then the response to 'dog' might be 'animal' or 'pet' or 'mammal'; that is, the name of the class. If the subject is constrained in the same way for several trials, the 'set' gradually establishes itself. This shows in two ways: the reaction times become shorter; and introspections show that the process becomes more automatic. Whereas at first the subject finds a correct response by trial and error, eventually correct responses tend to appear as soon as the stimulus word is given. The present experiment illustrates this building up of an unconscious determining tendency.

Materials. Sixty nouns, excluding abstract nouns, not in the order in which they came to mind. Assign numbers from 1 to 60 to the nouns, and arrange them in an order determined from a table of random numbers.

Method. The first thirty words are presented to the subject in sequence as stimulus words. In each case S must reply with the first word which it makes him think of (*un*constrained association). Reaction times are taken in each case.

The second thirty words are presented in exactly the same way, but with the instruction: this time I want you to reply with the first word which comes to mind which *belongs to the same class* as the word I give you. For instance, if I say cat you might say dog, since both cats and dogs are pets. Or to chair you might say sofa. Do you understand? (Explain further if necessary.) All responses are again timed with a stop-watch. For these constrained associations, after each of the *first* five responses and again after each of the *last* five ask the subject for his introspections (or rather retrospections) on what occurred to him between the giving of the stimulus word and the response. (One disadvantage of introspection is that what is being reported on – the introspector's thoughts – must be altered by the very act of introspecting. Retrospection, as used in this experiment, may partly avoid this; but it is still possible that the subject's thoughts may be affected by the knowledge that he will be required to retrospect. Also, in the brief time interval between the thinking and the retrospecting, something may be forgotten or misrepresented.)

Treatment of results. Reaction times. The hypothesis is that these will get shorter for the constrained associations, but not for the unconstrained. This can be tested as follows. For the unconstrained associations, compare the mean of the last 10 reaction times with the mean of the first 10 (t-test is suggested for the purpose). Similarly for the constrained associations. The first test should yield non-significance, the second should be significant (Hoel 1960).

If negative results are obtained, there are two main reasons: (1) the reaction times are so variable that much larger samples would be needed to demonstrate a significant difference between means, (2) the determining tendency may have established itself too rapidly. This happens with some subjects.

The means of all reaction times under constrained and unconstrained conditions may also be compared.

Retrospections. Compare the retrospections for the first and last five of the constrained associations. Is there evidence that trial-and-error thinking gives place to an automatic reaction?

Rationale of design. Any shortening of reaction times during constrained association might be the result, not of a determining tendency becoming established, but of experimental sophistication in the subject. For this reason, 30 unconstrained associations are included as a control. Where should they occur – before, or after, or before and after the constrained associations, or in some other way mixed with them? There are several considerations:

1. For the determining tendency to show its effect, the constrained associations should occur in a series without a break.

2. If the unconstrained associations are divided, 15 before and 15 after the constrained associations, fatigue might obscure the occurrence of improved performance in the unconstrained associating.

3. In the design as proposed here, any progressive effect such as fatigue, or transfer of training, are likely to *go against* the effect which the experiment attempts to demonstrate. This is preferable, since if the results are in the predicted direction, one can be sure *a fortiori* that they are due to the factor being demonstrated, and not to the other progressive factors which the design attempts to control.

SUMMARY OF RESULTS FROM INTROSPECTIVE METHODS

Those things about thinking which have been discovered from the introspective reports of subjects can be summarized as follows:

1. There are large differences between individuals in what they are able to report, and the kinds of things which they say happen while they are thinking. It has been seen that these differences are marked with regard to the imagery which occurs, and the extent to which thinking is said to occur without any images at all. Individual differences also show in the ways people go about solving problems. This is seen very nicely in the game known as twenty questions. When faced with the problem of finding out what somebody is thinking by means of questions which are to be answered by 'yes' or 'no' only, no two people use exactly the same strategy.

2. Most people are able to report associative links in the sequence of conscious thoughts which they have, but only for some of the time. The 'laws of association' were built on the assumption that these links are always there.

3. Most thinking (probably including phantasy thinking) appears to be directed; that is, one thought follows another not just by chance, nor just because of association, but also because of an underlying directing agency which is not usually analysable by introspection. This direction can often be attributed to two main sources: the goal which is set by the accepted problem, and the existing state of the person. For instance, if I am asked to complete the sentence 'I should very much like to . . .', I will start thinking about this sentence, and will try to find words which will make it meaningful, since this is the goal which has been accepted, but the solution I finally arrive at, 'I should very much like to have a cup of coffee', is the result also of my state of thirst at the moment. Both these factors are aspects of my 'set' at the moment.

4. In solving problems, and sometimes too in creative thinking, many people report sudden changes in thinking which often go with seeing things from a new viewpoint. There is a special class of these changes which leads to a solution or partial solution of a problem. In such cases one talks of having a flash of insight. Köhler (1925) considered that he was studying such sudden changes when his apes would solve a problem after a period when they looked as though they had been thinking about the problem.

5. There are great differences in the 'degree of insight' which one may have while thinking. When first posed with a problem, thinking may at first be chaotic and only gradually give way to some structure or patterning. This change need not be sudden. On the other hand, if the kind of problem is familiar, the thinking may be structured and goal-directed from the start. It is thinking of such a structured kind that Köhler (1920, chap. 10) calls insightful.

6. Sometimes thinking of the problem-solving kind is less efficient if the thinker tries too hard. Many people report this when trying to solve crossword puzzle clues, and especially anagrams. This seems to be equivalent to a similar finding in the performance of motor skills. On the other hand some motivation is of course necessary.

7. Many people have reported a period of 'incubation' in problem-solving and in creative thinking. This is usually characterized by thinking about the problem intensely, then putting it on one side, whereupon a solution or illumination may occur, as though thinking had proceeded unconsciously.

8. Thinking makes use of 'lower order habits' in much the way that motor skills do. Early work on typewriting and the sending and receiving of morse code (Book, and Bryan and Harter, in Woodworth, 1938) showed that skills may be hierarchically organized. Operators learn simple skills related to individual letters and then words before they become skilled at the whole task, and these simple skills must become automatic or habitual if progress is to be made. The same process shows in the acquisition of a foreign language. At first the meaning of each word in turn is learnt, but eventually such word-to-word translation becomes automatic and thinking in the foreign language may become possible (Foss 1956).

There is no doubt that language plays a large part in most people's thinking (here, of course, we are talking about the native tongue).

Although there is evidence from brain-injured patients and also from deaf-mutes that thinking without language can go on, it is clear that in the normal subject thinking without words is colourless and lacking in logical sequence. Whenever thinking involves words it is clear that 'lower order habits' are being used, since the words stand for things (including processes) and this 'standing for' is something which has had to be learnt, and which was at one time not automatic but a matter of conscious effort. Mathematicians are able to give many examples of the way in which their thinking has this hierarchical structure, though in this case the habits are not necessarily verbal. There is good reason to believe that in all thinking the thinker makes use of symbols which stand for whole classes of objects, attributes, operations or processes, and that this gives rise to economy (but also inaccuracy) in thinking. This will be discussed again in the introduction to the third experiment in this chapter.

MOTOR THEORIES OF THINKING

From the above it is clear that the introspective method has provided a wealth of information about what happens in thinking, but the method does not allow for experiments which will produce measurable results, or enable principles and laws to be set up in a respectable scientific fashion. This has led experimental psychologists to adopt a behaviourist approach to the study of thinking. Thinking is regarded as one aspect of behaviour. Subjects are not asked to introspect, though what they say in answer to questions may be taken at its face value if there is reason to believe that the subject is co-operating.

This behaviourist approach has been strengthened by the widespread belief that thinking is in fact 'internalized' behaviour. The idea is that, when one is thinking about ways to solve a problem, one is *implicitly* indulging in trial-and-error behaviour – making the movements which would be involved in the behaviour, but on a minute scale, or saying the words sub-vocally. A considerable number of experiments have been performed which claim to show that thinking is actually accompanied by small muscle changes in the periphery of the body. For instance, Jacobson claimed to have shown this (1929) for the skeletal muscles as well as for speech muscles and Max (1934) showed it for muscles of the fore-arm in deaf-mutes who had learned to communicate by gesture rather than language. Some theorists have taken the extreme viewpoint that such muscle changes are a necessary condition for thinking. This is known as the motor or peripheral theory of thinking. A more moderate view is that they facilitate thinking, but are not essential. Others have argued that these muscle changes are not essential but are the result of some kind of overflow of more central activity. It is possible that such movements play more causal a part in the thinking of the young and it is not surprising to notice that the very old may return to making speech movements when reading or thinking.

It is possible that some of the muscle changes which have been observed are the result of a general heightening of activity and most experiments have not been designed to test this alternative. A clear demonstration of the motor theory would need to show that certain patterns of muscle changes always accompany certain kinds of thinking in a given individual, and that these patterns do not occur otherwise. The technical difficulties involved here are enormous. In the experiment which follows the problem is attacked obliquely in a way which makes it possible in the undergraduate laboratory. It is of interest to try the experiment on several subjects. The results will show that there are large individual differences between people in the extent to which speech movements do or do not play a part in their verbal thinking. These differences bedevil work on this topic. It is possible that the motor theory of thinking holds only for certain extreme types of people.

EXPERIMENT II. EFFECTS OF INCOMPATIBLE SPEECH MOVEMENTS DURING THINKING

Teaching laboratories do not have the equipment necessary to measure the minute muscle changes which occur during thinking. To demonstrate that these changes do exist and to investigate their place in the thinking process it is necessary to use indirect methods. Several such methods exist, though all of them have certain deficiencies so that the results obtained are not entirely unambiguous.

Jacobson (*op. cit.*) was one of the first to use an indirect method. He was a pioneer of progressive relaxation, using the method for therapy as well as for research. Using this method he was able to reduce subjects to a state of general relaxation which is not normally possible without special training. When subjects were in such a state, he claimed that their thinking was impaired, that they were inefficient at solving simple problems, and were unable to imagine themselves making movements without becoming unrelaxed and to some extent making those movements. 'The experience of muscular tenseness (is) a *sine qua non* of . . . thought process' (Jacobson, p. 186). Another method which has been used to prevent muscle movements is to make subjects hold a pencil between their teeth while carrying out tasks which might be expected to involve the making of speech movements. It was expected that gross speech movements would be prevented by this technique (Barlow 1928). Rather surprisingly the experiment gave positive results. It would have been expected that some speech movements, especially movements in the larynx, were still possible; also there is evidence that a certain amount of tension assists certain symbolic activities (Courts 1939).

In the present experiment, an attempt is made to demonstrate the part played by speech in thinking by making the subject carry out a routine task – repeating the alphabet over and over again – while he is thinking. The organs of speech are thus occupied with a task which is incompatible with their playing a part in the thinking process. If muscle movements are playing any major part in the thinking process, it will be predicted that repeating the alphabet should grossly impair any thinking process in which verbalizing is important. If it does not, the following explanations arise:

1. Muscle movements do not play a major part in thinking, and they may possibly represent an 'overspill' of central activity;

2. Muscle movements are still important, but subjects are able to rely on muscles other than those associated with speaking even when thinking about verbal material;

3. Muscle movements are still important, but in carrying out two simultaneous verbal tasks, subjects are able to alternate, so that the muscles associated with speech 'devote' themselves to the thinking activity in very small intervals which may exist while carrying out a routine process like saying the alphabet. In this case one would expect both tasks – the thinking and the saying of the alphabet – to be slowed down.

If repeating the alphabet is effective in impairing a simultaneous thinking task, it could be argued that this is the result not of incompatible speech movements, but simply of the distraction provided by the second task, since in general it is difficult to *do two things at once* if it is necessary to attend to both. It is therefore important that the experiment shall be designed so that the distracting effects of reciting the alphabet can be assessed separately from those caused by conflicting speech movements. This is done by comparing the impairment caused by the distracting task on *three* other tasks: (1) the crossing of lines – a routine task in which there is likely to be no verbalizing by the subject, and in which any impairment can be taken to be due simply to the distracting properties of reciting the alphabet; (2) the cancelling of the word 'the' whenever it appears in a passage. The movements made by S are similar to those in task 1, but the perceptual problem is more difficult – one particular word has to be recognized whenever it appears. This is a task not requiring much intelligence, but one in which some verbalizing is likely to occur; (3) the writing of meaningful English. S may write anything that occurs to him, but it must be meaningful. This is a very simple task indeed, nevertheless it is essentially verbal (the quality of the handwriting is not considered, though it may be wise not to call the subject's attention to this!).

In this experiment it will be expected that all three tasks will be affected by reciting the alphabet, through its sheer distracting power, but that tasks 2 and 3, and especially task 3, will also show effects of incompatible speech movements.

Materials needed. A stop-watch.

Task 1. Quarto sheets of short vertical lines arranged in rows. The subject's task is to cancel every other line by drawing a short horizontal line through it as quickly as possible.

Task 2. Duplicated copies of passages from newspapers, typed onto quarto sheets. All passages should be roughly in the same style of English (so that the word 'the' will occur with roughly the same frequency in all passages). The subject's task is to cancel with a horizontal line the word 'the' every time it occurs and as quickly as possible.

For task 3, plain quarto paper only is needed.

Design. Each task must be given with and without the simultaneous recitation of the alphabet (which the subject is to do continuously and aloud), so that impairment can be assessed. It is suggested that S be required to work as fast as possible under all conditions (and that he be continually reminded of this) and that his performance be measured by the amount he does in a set time interval. Since during the course of this experiment there are likely to be progressive factors at work such as learning and fatigue, it will be necessary to repeat each task several times and take average scores. This will also be necessary for statistical treatment. In the following suggested design, 1, 2, 3 stand for tasks 1, 2, 3; A stands for the condition where S repeatedly recites the alphabet aloud, X for the condition where he does not. Each task should be carried out for 15 seconds only.

Order of tasks. 1X 1A 2X 2A 3X 3A 1A 1X 2A 2X 3A 3X. After a short time interval, this should be repeated in the reverse order. Scores will then be obtained four times for each task under each condition. S will have spent only 6 minutes actually performing, so that it should be possible to repeat the design two or three times. This would make statistical treatment of the results possible.

Practice trials. The learning which takes place at the beginning of any novel task is likely to be large. It is wise therefore to give S practice trials with all three tasks before starting the experiments proper, so that the first few scores will not be over-influenced by learning.

Instructions to subjects. Task 1. When I say 'start' I want you to cancel with a short line every other of these vertical lines (*demonstrate*). When you reach the end of a line, carry on with the next until I tell you to stop. Work as fast as you can.

Task 2. Here is a passage taken from a newspaper cutting. When I say 'start', begin at the point I have marked and cross out the word 'the' every time it occurs. Work as fast and accurately as possible until I tell you to stop. (Do not allow S to start reading the passage beforehand.)

Task 3. When I say 'start' I want you to write down anything you like, so long as it is meaningful English, arranged in sentences. Continue until I say 'stop', working as fast as possible but keeping your writing legible.

Condition A. While you are carrying out the task, I want you to repeat the alphabet aloud. When you have finished it, start again. Do not make any breaks, except where necessary for breathing.

Condition X. Under this condition the S may mutter to himself if he wishes. Presumably the muttering will be related to the task and not incompatible with it.

During the course of the experiment, S will flag in his reciting, and will tend to make more pauses than are necessary. When this happens he should be brought to order.

Scoring. Task 1. Count the number of lines which S has cancelled in the 15-second period.

Task 2. S indicates on each occasion how far he has got with reading the passage. Three scores can be used: number of words scanned; number of times 'the' has been cancelled; number of times 'the' has been missed.

Task 3. Count the number of letters (not words) S has written. If S has written nonsense, and made spelling mistakes, draw this to his attention so that he is warned to keep up the standard asked for in the instructions.

Treatment of results. For each task under condition there will be four (or a multiple of four) scores. The hypothesis that reciting the alphabet interferes with performance of a task can be tested by comparing the scores for any one task under condition A with the scores for the same task under condition X, using a t-test or a non-parametric technique. If the null hypothesis is not confirmed, this will suggest that there is interference for that task. For task 2 this test can be made on all three kinds of scores remembering, of course, that the hypothesis suggests that the omissions will be more under condition A, whereas number of lines and number of cancellations will be less.

It is likely that the results will be significant for tasks 2 and 3, but not for 1. This would support the hypothesis. But supposing all the results are significant, or all are non-significant, it is still required to test whether the interference effects are maximum on 3 and minimum on 1. A rough way of doing this is to work out an 'impairment score' for each task, in terms of the ratio $\frac{\text{performance in condition A}}{\text{performance in condition X}}$. For instance, for task 2 the ratio might be $\frac{\text{total words read under A}}{\text{total words read under X}}$. By using totals, only one 'impairment score' would be obtained for each task, so that statistical comparison would not be possible. There would be only an indication if the impairment scores tended in the predicted direction.

Discussion of results. Let it be supposed that the scores have supported the hypothesis: that simultaneous recitation of the alphabet while performing a task impairs performance on that task, and that the impairment is greatest when the task is a verbal one. Can it now be said that this impairment of verbal tasks indicates that speech movements play a major part in verbal thinking? Unfortunately there is still an alternative hypothesis, which must in fact take priority. It is that, when two tasks are performed simultaneously, the amount of impairment of the first task caused by the second becomes greater the greater the complexity of the first task. If everyday distractions are considered, this hypothesis will be seen to be sensible. Task 3 is more complex in several ways: it is the only task of the three which requires the subject to create something, and (probably very important) it is a *continuous* task. Tasks 1 and 2 can be carried out intermittently – it is possible for the subject to cancel a line or a word in a brief interval during the reciting of the alphabet – rather in the way that one may answer a questioner in the middle of reading a story without losing the thread of the story; but task 3 is much more demanding in that the subject is required to produce a continuous passage of prose.

Is it possible to decide between these two hypotheses? Is the greater impairment of task 3 due to its greater complexity or to its verbal character? One answer to this question would be to conduct a subsidiary experiment in which a non-verbal complex task was used. One of the difficulties here is to equate tasks for complexity. If one relies on subjective impressions of difficulty, then a suitable task might be 'mirror drawing' – the tracing of a line between the double contours of a star which can be seen only in a mirror. This task has been used very much in psychology, because it provides a complex motor activity which can be produced with simple means. For the purposes of the present experiment it is useful because it can be supposed that most subjects will use words hardly at all while carrying out the task. The usual score taken to assess performance in mirror drawing is the time taken to trace completely around a double-contour star. The measure of impairment will therefore be

$\frac{\text{time taken under A}}{\text{time taken under X}}$. Unfortunately, subjects always show a very great amount of learning with novel motor skills of this kind, and the dramatic improvements in performance which result from practice make such tasks difficult to use for the sort of experiment being discussed here. One way out of this is to practise the subject until he reaches a plateau of learning at the skill before starting on the experiments; but the result of this is that, in a sense, the task is no longer complex – it has acquired some of the properties of a habit and is no longer ideal for the present experiment.

Another way of attempting to decide between the two hypotheses is to use an interfering task which is so simple that its distraction properties *per se* can be shown to be negligible. One such task is to get the subject to say over and over again one single word, preferably one which keeps the lips and tongue fully occupied, such as the word 'with'. If the constant repetition aloud of the word 'with' is used as an interfering task, it will be found that performance at non-verbal tasks is not affected measurably, but that verbal tasks, such as task 3, are still severely impaired. A better test of the original hypothesis would therefore be to use the constant repetition of such a single word rather than to use the more complex task of repeating the alphabet. The reason why this was not suggested originally is that very few subjects can do this without cheating. Most subjects, if asked to repeat over and over 'with' while they are writing free prose, will modulate their utterances so that what they are speaking sounds very much more like what they are writing! So that their speech movements have ceased to be incompatible.

CLASSIFYING

Earlier it was stated that '. . . there is good reason to believe that in all thinking the thinker makes use of symbols which stand for whole classes of objects, attributes, operations or processes . . .' It is possible that this sentence could be applied, with little modification, to perception and remembering as well as to thinking. If someone reports that he has just seen a dog, he is using such a classifying process. He is reporting seeing one of a class of objects. It is quite likely that the only attributes of this particular dog which he may remember are just those attributes which apply to all dogs – four-leggedness, hairiness, etc. Remembering in this way is economical: it means that the dog in detail has not had to be retained, and the 'load' on the nervous system is much reduced. But this is done at the expense of accuracy and there is loss of information. For remembering to happen in this way several things are necessary: the observer must already have a classification of doglike objects; he must perceive an object which has sufficient attributes of a doglike kind to result in that object falling in the class; and he must have a word for the class – 'dog' – so that he can later report having seen such an object. Of course, most people have many subclasses of dog: small dogs, shaggy dogs, terriers, etc., and if one of these subclasses is used in remembering a dog there will be less loss of information and less inaccuracy in recalling the dog. To reach such a degree of sophistication more learning will have had to take place (presumably this will be seen at its maximum in a dog fancier) and greater discrimination in perception will be necessary.

During thinking, it is clear that classifying processes are being used continually. On being given a problem to solve, one response of the subject will be 'what kind of problem is this?', that is, what class does this problem belong to? During the solving of the problem it is likely that the subject may frequently use analogy (i.e. conscious transfer) – 'this is like a pendulum whose swings gradually get smaller'. He is almost certainly not thinking about any particular pendulum but about the class of objects called by that name. If he visualizes one, it may be none that he has ever seen, but an imagined 'typical instance' of the class. And when he produced a solution, he will call it a solution because it generates a feeling of certainty which is associated with having solved problems, that is, it will be a feeling that falls into a certain class.

The word classifying as used here could cover many psychological processes, from stimulus generalization and stimulus equivalence to concept formation, concept attainment and categorizing. When a subject in a conditioning experiment has been trained to withdraw his hand to note G on a xylophone, he will also withdraw his hand to F. He has 'classed' the two notes together as far as this particular response goes, but the two notes are on the same continuum and indeed very like each other as physical stimuli, and here the expression 'stimulus generalization' is used to describe the process. On the other hand, a man may think of a dog if he sees one, hears one, reads the word 'dog', hears someone speak the word, etc. Here the various stimuli which produce the same response are very unlike each other and do not even belong to the same stimulus modalities. In such complicated cases we say that the man has a concept of dog. Nevertheless this is still a classifying process.

This process seems to be so important in many kinds of behaviour that it has been studied at many different levels. As it concerns thinking, the following are some of the many studies of classifying:

Concept formation in children: how children form concepts of number, volume, etc., and at what ages. Much of Piaget's work is concerned with this.

Categorizing in children: how children sort objects into classes, and on the basis of what attributes (Piaget, and Werner, 1948).

Concept attainment: this has been studied in many experiments in which subjects have to arrive at a way of classifying objects which the experimenter has thought of (Hull, Reed, Smoke, Heidbreder, Hanfmann and Kasinin. See Andrews (1948) pp. 108–15). This is also often called concept formation, but it has been pointed out that the subject is using concepts which he has already formed.

Effects of brain lesions: Goldstein (1942) was one of the first to point out that brain-injured patients find it more difficult to use abstract concepts than concrete ones, and often lose their use completely.

Schizophrenic thinking: one of the most noticeable things about schizophrenic thinking is the way in which items are classified idiosyncratically. Arieti (1948) quotes the case of a schizophrenic who had classified together sex, Jesus Christ and a cigar box because they all involved encircling. Creative geniuses also arrive at unusual classifications, but these are not thought of as psychotic if their scientific or artistic validity can be demonstrated. For instance, to class together an apple falling to earth and the earth in orbit around the sun was highly unusual, but not psychotic.

Individual differences in categorizing: a series of experiments under Bruner is directed at investigating individual differences in the sizes of category which people use, in relative abilities to use conjunctive and disjunctive categories, etc. (Bruner *et al.* 1956).

Logical thinking: many problems in logic are problems in the manipulation of classes or categories. A problem of the kind 'All A are B; some B are C; therefore some A are C – true or false?' is getting at a person's ability to manipulate categories. Woodworth and Sells (1935) have worked on syllogisms in this way. Classical logical forms such as the syllogism are rare however in everyday thinking.

Several of the experiments on concept attainment have used a technique which is similar to that known as paired-associate learning. Pictures or figures are prepared, a group of which can be classed together because they have something in common. To each of these pictures or figures a symbol is attached (e.g. a nonsense syllable) and the subject has to learn what property of the picture or figure is represented by the symbol. In Smoke's (1932) experiments, in which geometric figures were used, nonsense syllables were attached to all those figures which had one property in common. For instance, syllable *dax* was attached to all figures containing a circle and two dots, one dot being inside the circle, the other outside. All figures not having this property were called *non-dax*. Smoke discovered that the negative instances assisted subjects in discovering the common property, yet at the same time subjects paid singularly little attention to these negative instances. Other experimenters have also shown that humans appear to avoid looking for negative instances when trying to identify concepts, even when the discovery of such instances might be of great assistance in solving the problem (e.g. Bruner, *op. cit.*).

The following experiment illustrates a technique, similar to Smoke's, for investigating concept attainment. It is applied to the problem: what proportion of positive and negative instances of a concept should be presented for greatest efficiency in arriving at the concept?

EXPERIMENT III. POSITIVE AND NEGATIVE INSTANCE IN CONCEPT ATTAINMENT

The problem. A subject is presented with a series of geometrical figures, some of which have a property in common, and are labelled X, others lack this property and are non-X. The subject is asked to guess what X stands for. Will he arrive at the solution most rapidly if all or nearly all the figures are X, or if half are X and half non-X, or if nearly all are non-X? In fact, what is the optimal percentage for positive instances $\left(\frac{X}{X + \text{non-X}} \times 100\right)$? This is an exploratory experiment. Results will to some extent be specific to the subject used in the experiment.

Considerations in the design. The optimal percentage of Xs will depend on the total number of instances, positive and negative, presented to the subject. This total will be controlled, kept constant, at 10 in all parts of the experiment. The percentage of positive instances will of course be varied, since this is the independent variable. The dependent variable – the speed of the subject at arriving at the concept – will be measured in two ways: time taken; and number of guesses to the correct answer. A main source of variance in the results will be the uneven difficulty of the varying concepts used. In the space of a laboratory class experiment it is not possible to conduct a preliminary investigation to help in choosing concepts which are all equally solvable. Hence it will be necessary to treat several concepts at each value of the independent variable, and use average results. If one subject is to be used throughout, progressive effects, such as fatigue, or transfer, are likely to occur, so that the various values of the independent variable must be presented to the subject in random order. Since the experiment is directed at finding the values taken up by the dependent variable (time taken) when the dependent variable (percentage positive instances) is given several values, the results may be treated by plotting time against percentage in a graph.

*Material.** Each problem to be presented to S will consist of a sheet of paper containing 10 geometric figures; those which have the common feature which S has to guess will be labelled X. The remaining non-X figures must, of course, not have this feature.

Fifteen such concepts will be needed. There are a great many possible concepts. Some are:

X	*non-X*
Containing at least one curve	No curves
Made up of three items	With 1, 2, 4, 5, etc. items
Containing an acute angle	No acute angles
Symmetrical	Asymmetrical
With lines crossing	No lines crossing

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* Sample figures which have been found satisfactory in use will be found in the instructor's material (Messrs Methuen & Co. 2*s*. 6*d*.).

The first step is to invent fifteen of these, trying, as far as this can be anticipated, to make them of the same order of difficulty. Label these concepts 1 to 15. These are to be apportioned randomly to the five values of the independent variable which are to be used.

Condition	A	B	C	D	E
Independent variable (percentage Xs)	20	40	60	80	100
Number of Xs	2	4	6	8	10
Number of non-Xs	8	6	4	2	0

There will be three concepts for each condition. The fifteen sheets of paper are then prepared according to this plan. If for instance, the concept symmetrical has been allotted to condition B, four symmetrical (labelled X) and six asymmetrical (labelled non-X) figures must be drawn on one sheet of paper. The arrangement of X and non-X figures should be varied from sheet to sheet, but always uniformly spread, so that S, when rapidly scanning the sheet, will easily spot examples of X and non-X.

The 15 sheets are now presented to S in a sequence prearranged so as to control progressive effects. For instance:

Order	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Condition	C	A	B	E	D	E	A	D	B	C	D	B	C	E	A

(Notice that for any one condition the ordinals add to 24. This means that for each condition the *average position* is the same.)

Instructions to subject. Here are ten figures. Those marked X have something in common which the others do not have. I want you to guess what that thing is, and go on guessing until I tell you that you have guessed correctly. (If S is not familiar with this kind of problem, give practice with a sheet which is not to be included in the main experiment.)

Measurements. Take the time for each problem with a stop-watch. Count the number of guesses which are made on each problem.

Treatment of results. For each condition, calculate the mean time taken for solving the three problems, and plot this against the percentage of Xs (positive instances). In general, the time for solution is shorter as the percentage of positive instances increase, provided there are still some negative instances. In the experiment described here, the optimal percentage is likely to be 80. If S has had to make several guesses before arriving at correct solutions (this will not be the case if the figures have been prepared thoughtfully so that each problem has a unique solution), the average number of guesses can be plotted against conditions, and a similarly shaped graph should result.

In this experiment, pooled results from a class may be plotted provided all experiments have been done by the same method, but preferably with different subjects being set to solve different selections of concepts.

REFERENCES

ANDREWS, F. M. (1948). *Methods of Psychology*. New York.

ARIETI, S. (1948). *Psychiatry*, **11**, 325.

BARLOW, M. C. (1928). *J. exp. Psychol.*, **11**, 306.

BARTLETT, F. C. (1958). *Thinking*. London: Allen & Unwin.

BRUNER, J. S., GOODNOW, J. J., and AUSTIN, G. A. (1956). *A Study of Thinking*. New York.

CARPENTER, W. B. (1874). *Mental Physiology*. London: King.

COURTS, F. A. (1939). *J. exp. Psychol.*, **25**, 235.

FOSS, B. M. (1956). *Acta. Psychol.*, **12**, 192.

GOLDSTEIN, KURT (1942). *After-effects of Brain Injuries in War*. New York.

HADAMARD, J. (1945). *The Psychology of Invention in the Mathematical Field*. Princeton.

HUMPHREY, G. (1951). *Thinking: an Introduction to its Experimental Psychology*. London: Methuen.

JACOBSON, E. (1929). *Progressive Relaxation*. Chicago University Press.

KÖHLER, W. (1920). *Gestalt Psychology*. New York.

KÖHLER, W. (1925). *The Mentality of Apes*. New York.

MAIER, N. F. R. (1930). *J. comp. Psychol.*, **10**, 115.

MAX, L. W. (1934). *J. gen. Psychol.*, **11**, 112.

McKELLAR, P. (1957). *Imagination and Thinking*. London: Cohen.

OVSIANKINA, M. (1928). *Psychol. Forsch.*, **11**, 302.

SMOKE, K. L. (1932). *Psychol. Monog.*, **42**, No. 191.

WERNER, H. (1948). *Comparative Psychology of Mental Development*. Chicago University Press.

WOODWORTH, R. S., and SELLS, S. B. (1935). *J. exp. Psychol.*, **18**, 451.

WOODWORTH, R. S. (1938). *Experimental Psychology*. New York.

Chapter 7

EXPERIMENTS ON ANIMALS

J. A. DEUTSCH

Stanford University, U.S.A.

INTRODUCTION

It has been the tradition among theorists in psychology to construct systems whose predictions are then tested by experiments done on the rat. This has had two unfortunate consequences. First, rat psychology has become a substitute for comparative animal psychology, and second, the rat's behaviour is almost unknown outside the conditions about which the psychologists had made predictions. It is therefore necessary to remove some misapprehensions at the outset. This chapter is not really devoted to animal psychology but to the behaviour of the rat, which in many ways is not a typical animal (if any such beast there be). The rat is a small nocturnal rodent, with numerous predators, and it is also an animal which lives in burrows. The wild *Rattus Norwegicus* is greyish brown in colour and is of an extremely vicious disposition. Both the albino white rat and the hooded rat are mutant varieties of the wild grey Norway rats and are genetically much tamer. It is these mutants which are consequently used by the experimenter, the 'better' specimens being selected for breeding. In this way a strain which can safely be handled and which performs well in the experimental situation has been selected. The rat which is used has undergone a process of 'domestication'. As a result, when we experiment on our laboratory stock, much of the behaviour we observe had already been built in by our choice of mutant and our further selection of animals. For the effects of genetic constitution on the behaviour of rats it is instructive to read the work of Keeler and King (1942). Such considerations should make us circumspect about any generalizations to other species from work on the laboratory rat. We certainly cannot say that we are studying animal psychology in general and it may be doubted, on account of the selection procedure mentioned above, whether we are even studying rat psychology in general.

The second point which was made above should also be amplified. It was said that the Rat's behaviour was almost unknown outside the conditions about which psychological theorists had made predictions. Theories were constructed chiefly about learning and deductions from them were tested by designing experiments on the rat. As a result, the behaviour of the animal outside these preconceived situations has been very little studied. The rat is kept in its metal cage until required for the experiment and then promptly returned after highly selected pieces of its behaviour are observed with great precision under the experimental conditions. Consequently it came as a surprise to the author that the laboratory rats are indefatigable burrow constructors when he came to observe them under more natural conditions. No reference to this behaviour is to be found in the literature.

It may be asked why the rat has enjoyed such high popularity as an experimental animal since Small began using this animal in psychological experiments in 1900. Among the factors which led to the choice of the rat as apart from other animals must be ranked the large size of its litters, the shortness of its pregnancy (21 days) and the fact that a rat is ready for experiment when it is three months old. Further, rats are small, a number of specimens can be kept in one cage (they do not normally fight each other, as will other small rodents) and they are on the whole free from epidemics. In short, large numbers necessary for the statistical nature of most psychological experiment are easily maintained. Added to these characteristics, is the rat's temperament. They are lively, inquisitive and highly adaptable (unless they are terror-stricken because they have been poorly tamed). They are cheap to buy and maintain and are very robust. When one has attempted to work with other species, as the author has with ferrets and squirrels, one begins to appreciate the virtues of the rat. As a result of these advantages, a large body of data has been obtained on selected aspects of the rat's behaviour, These have been chosen as a result of the various controversies which have raged in psychology and cover the learning processes of this animal with an unusual thoroughness. It is safe to say that we know much more about the learning behaviour of rats than of any other animal. Also as a result of the theoretical interest of many of the observations and their scrutiny by opposing sides, many issues have been investigated with a thoroughness unusual in other parts of the subject, so that the range of theoretical possibilities has been narrowed to the point where close thinking becomes rewarding. Though the organism that has been studied has partly come to be what it is by selective pressures applied by the laboratory, all animals are the resultant of selection operating in one way or another. There is the hope that if we come to understand thoroughly one animal as such, other species will prove less refractory to theoretical investigation. However, even if we study the rat for its own sake, a set of problems of extraordinary fascination clamour for solution.

NOTES ON HANDLING RATS

Unlike human subjects, rats have to be kept in the laboratory and looked after by the experimenter. Though this has certain inconveniences there are compensating advantages. The rats are always there when required, and to quote Tolman (1945): 'They do not go on binges the night before one has planned the experiment.' Keeping a small colony of rats is not a difficult business but it is a task which demands close routine. Such manuals as Farrer-Griffith or the *UFAW Handbook* give excellent information. For use as animals in practical classes there are various practices which have been found expedient. The laboratory should do its own breeding. In this way the animals used for the practicals can be handled from the time they can walk – and before they have a fear of handling. An animal, once it is afraid of handling, can be tamed, but is never so trustworthy or relaxed as the animal which has always been tame. A rat is not born afraid of human hands. Such a fear seems to appear at a certain stage in its development. If it is handled before this crucial stage this fear does not appear when the familiarity is kept up by constant handling. In this way really tame animals can be obtained. It is necessary to instruct the animal attendant to take the young out for a quarter of an hour each day and to handle them. In this way, when the animals are three months old they are ready for use in an experiment without any pre-training and with safety. This aspect of safety is worth stressing. A rat has very sharp long teeth and its bite can produce various types of illness. It is therefore well worth taking certain precautions. Animals should never be given tit-bits by hand. Rats are half blind and greedy and may mistake a student's finger for a tit-bit. Hands should always be washed, not only after, but before the animals are handled. The rat identifies its food chiefly by smell and if the hand smells of food it is treated as food. Care should be taken that the animal is not hurt. It is to be remembered especially that the rat has a long tail which is easily trapped. When the rat is hurt, from whatever cause, it will turn on the nearest object (preferably a moving one) and bite it with devastating effect. Therefore if a rat is accidentally hurt, do not attempt to lift it with your hand. Your hand will be bitten however good your intentions. Use another instrument.

However, unless the rats have been thoroughly tamed by the method described above, bites will occur when attempts are made to pick the animals up. Instead of using a thorough taming procedure it is possible to use thick industrial gloves to protect the hand. This method, though guaranteeing safety, does not provide good experimental subjects. The rat cannot do any damage by biting but it is likely to be in a frightened and disturbed condition, and in no fit state for an experiment, as fear tends to produce immobility in the rat. The wild rat is hunted in its natural state by predators which, like the cat and the fox, are visually sensitive only to moving objects. The rat can therefore best hide by keeping motionless. And this, unless we are very careful, it will attempt to do if it is placed in the maze without being thoroughly tame. The rat should always be treated as a pet, gently but firmly.

Many experimenters insist that the rat should be kept in noiseless surroundings and that the experiments be conducted in complete silence. Having seen rats live on railway stations unconcerned at the uproar, it has always seemed to the writer difficult to believe that rats are necessarily upset by noise. In fact the greater the precautions taken about noise, the greater the disturbance in the rat's behaviour which results from it. The truth seems to be that the rat is upset by any unusual stimuli, and it will be upset by noise if the noise is unusual. If a rat is well habituated to noise it will not take notice of it. When rats were first introduced into the writer's laboratory they were kept in a room next to the very noisy main road. As a result a rat trained for a demonstration to operate a Skinner box maintained its habit with perfect composure even when the room in which the demonstration was held became full of people milling round and talking loudly. Therefore if the rats are to be used in the practical class, they should not be made to lead too cloistered an existence before this event. Otherwise they will behave like the countryman on his first visit to a noisy city.

EXPERIMENT I

Drive discrimination in the rat. Introduction. An animal will only eat, or run a maze in order to eat, when it is in a certain state. That is when it has not had access to food for some time before. Similarly, it will not drink, or perform any instrumental act in order to drink, unless it has been deprived of water. Any theory accounting for animal behaviour must account for these facts. There have been many theories of behaviour such as Thorndike's, all stemming partly from the doctrines of associationism and partly from the work of Pavlov. The most generally accepted and best worked out of these theories has been put forward by C. L. Hull. This may be regarded as the best attempt to explain the facts of behaviour using the notions of conditioning.

It is one of the basic tenets of such theories that a response made by the organism, that is, some movement occurring in the musculature, is set off by some other set of events originating in the sense organs and called a stimulus. Therefore if an animal does something on one occasion and not on another, there must be a difference in the stimuli present on the two occasions. It is held in the case of the animal which does not eat when food is present that there is a stimulus absent. This stimulus is thought to arise from within the organism, unlike the stimuli of food, and persistently to send up a stream of afferent impulses which give rise to eating when they are present concurrently with the stimuli of food. These stimuli arising within the organism in the case of hunger are called drive stimuli (S_D). These drive stimuli (S_D) will in combination with the appropriate stimuli give rise to responses which in the past became associated with eating.

This is held to occur as follows. If a stimulus and response occur together in time (as they can by chance) and such a conjunction is followed closely by a reinforcing state of affairs, then when the stimulus occurs again it will tend to evoke the response. By a 'reinforcing state of affairs' is meant an abrupt diminution of the drive stimulus (S_D) which will occur for instance when the animal's physiological deficit, which caused the stimulus, is abolished, say, by the ingestion of food.

As the drive stimuli and the stimuli at a choice point in the maze occur together before the animal makes a response which is followed by a reward, the stimuli at the choice point and the drive stimuli will, when they occur again, tend to evoke the response which was followed by reward. However, this notion that *a drive, like hunger or thirst, is just another stimulus, similar to the stimuli generated by the maze*, soon met difficulties. Hull himself (1933) tested the notion by seeing whether animals can learn to respond in one way when they are, say, hungry, and in another when they are, say, thirsty, running in an otherwise identical situation. For example, if we take a T-maze and place water in the left goal-box and food in the right goal-box and make the animal thirsty one day and hungry the next, it should be possible in time to train the animal only to take the side appropriate to its need. The stimuli of hunger should tend to evoke the right turning response and the stimuli of thirst a left turning response. Hull accordingly performed such an experiment, but with one difference. He used a maze in which a turn one way would produce water and one the other produce food. However, both paths led to the same goal-box because the arms of the maze after diverging came together again. On a 'hunger' day the animal could reach the goal-box with food in it if it turned in one direction. If it turned in the other, it simply met with a block and had to retrace. Now the animals in this situation learnt extremely slowly. Hull found that there was almost no learning in the first few months of training and only about 77 per cent accuracy by the first 200 days of training with five trials a day if we take the first trial in the day. Such slowness of learning is to some extent inconsistent with the notion that the animal is being conditioned to differing stimuli in the ordinary sense. However

Leeper (1935) in a series of experiments was able to show that only animals in Hull's situation (with a single goal-box and a blind alley if they were unsuccessful) found the task of discriminating their drives difficult. He showed that if animals were placed in a situation where the food and water were placed on two different goal-boxes, so that they would see the food or the water whether they were right or wrong, would learn with great speed (90 per cent accuracy on 1st trial) after only 10 days. Bolles and Petrinovich have also demonstrated extremely rapid learning in this situation. This removes one particular difficulty for the Hullian view, by showing that if there are drive stimuli they can be efficiently discriminated as we should expect by analogy from other stimuli.

There are however other difficulties left. We have to explain why learning was so much more efficient in Leeper's experiment than in Hull's. In Leeper's experiment there were two goal-boxes and the animal never met a block whichever way it went. In Hull's experiment there was only one goal-box and the animal could not reach it if it made a mistake. There are therefore two possible explanations of the discrepancy between Hull's results and Leeper's results. The first is that the difference was due to the goal-boxes and the second that the difference was due to the blocking of an alley, which led to the goal and which the animal would remember and assume was blocked even when it became the correct alley. To find out which of these contingencies gave rise to the discrepancy, the present writer (Deutsch 1959) repeated Hull's and Leeper's procedure on two groups of rats and also added a third group. This third group received its rewards (food or water) in the same goal-box but found its way to the goal-box and the reward whether relevant or irrelevant, whichever way it had chosen. In this way it could be seen (*a*) whether the inferior performance of the Hull group was due to confusion because of the use of one goal-box or (*b*) whether it was the absence of the irrelevant reward or (*c*) whether it was the presence of a block when the animal had made an incorrect choice which led to the discrepant results. The result of this further experiment showed that the animal's difficulty stems from having to go a different way to the same place depending on what it needs. The results of the Hull and control groups were very similar, which showed that it was the factor of the common goal-box which led to the difficulty. However, there was a difference between the first trial scores of these groups. The Hull group was greatly inferior to the control group on the first trial of each day's runs, showing that the block had an effect too, but only on the first run. That this is the separate effect of a block and that it appears in other situations has been shown in the context of Leeper's experiment by Deutsch and Anthony (1958). These workers used a T-maze with food in one arm and water in the other. They also made their animals hungry or thirsty for each day's runs. They divided the animals into two groups, one which met a block (which was out of sight at the choice point) when they made a wrong choice and another group which did not. Again there was a large difference between the first trial scores of these groups. The animals which were prevented by a block from reaching the wrong goal-box did much worse on the first trial of the next day. The percentages of correct choices for the first trial of each day for the first period of training may be compared for the two groups. The experimental group with the wrong goal-box always blocked scored 61·52 per cent (SD = 12·17) correct on their first trials of each day whereas the control group scored 78·94 per cent (SD = 11·49) on the first trial of each day. The probability of this difference as assessed by the t-test is smaller than 0·01. However, if we compare all the trials together, then the percentage of correct trials for the experimental group with the group goal-box blocked off is 74 (SD = 6·24) and that of the control group (both boxes accessible) was 78·05 (SD = 3·18). The means are not significantly different here. (These findings, and those of the Hull-Leeper experiments, can be predicted by Deutsch's hypothesis (1956).) Though it is to be doubted whether Hullian theory can account for the findings quoted above, a more serious difficulty arises for the theory that a drive is a stimulus which is conditioned when we consider the experiment of Kendler. Now Kendler trained his rats when they were hungry and thirsty simultaneously. He used a T-maze with water on one side and food on the other. His experiment is very important with regard to the stimulus theory of drive because both the stimuli of thirst and hunger were present for Kendler's animals whenever they made a rewarded choice. Hull could only explain the animal's ability to choose correctly in his and Leeper's situation by saying that the drive stimuli of thirst had become connected to the response leading to water because they were present before this response occurred. The drive stimuli of thirst had not become connected to the response leading to food because they had not been present before that response took place and was followed by reward because the animal was then hungry and not thirsty. Now in Kendler's experiment the stimuli of hunger and thirst were simultaneously present. As a result the stimuli of hunger would be connected equally to the response leading to the food and to the water. The same applies to the thirst stimuli. Therefore when the animals were made separately hungry or thirsty they should not have been able to choose the correct reward except by chance. The drive stimuli of thirst appearing alone would, according to Hull's postulates, evoke both the response of going towards the food side and the water side impartially. (It is to be stressed that Hull when he talks about the reinforcing process treats all rewards as having the same effect. A thirst drive stimulus will be connected to a subsequent response whatever the nature of the reward, or diminution of need, which follows.) However, Kendler found that the animals which had been trained when both drives were present, could choose the appropriate side when one drive was present. This makes it look as if drives do not operate as if they were stimuli and that reinforcement is much more selective than Hull postulates.

Hull himself, following the suggestion of Spence, Bergmann and Lippitt, adopted another line of reasoning to help him out of his difficulty. He attempted to save the situation by introducing the notion of the antedating goal reaction or r_g.

This notion was introduced by Hull to account for the behaviour of an animal when it was not apparently responding to stimulation impinging at the time but was seemingly guided by a goal which was not directly stimulating the animal. This is what must be happening on the Hullian analysis in the Kendler double-drive situation. The stimuli impinging on the animal at the choice point cannot be determining the correct choice of the animal. It must be the goal towards which the animal is directed which is determining the animal's choice. Such a notion seems at first sight alien to Hull's way of thinking. But Hull attempted to derive this kind of concept from the principles of conditioning in the following way. He suggests that a part of the stimulation characteristic of the goal is acting at the choice point and that it has been displaced. It has been displaced because each stimulus when it occurs does not die away immediately but fades away gradually. Therefore the traces of the stimuli at the choice point will be attached to responses occurring much later in the sequence of events, and therefore these stimuli will tend to evoke these responses prematurely. Some of these responses will be those made by the animal in the goal. To quote Hull (1952):

> consider an organism which is presented with a sequence or chain of external stimuli, S_1, S_2, S_3, S_4 and S_G, and which makes a sequence of responses R_1, R_2, R_3, R_4 and R_G, where S_G is the food stimulus and R_G is the consummatory response, e.g. that of eating. The organism is assumed to be hungry so that S_{Dh} the drive stimulus of hunger will accompany the R_{Ge} or eating response. The preceding considerations show that R_G may be reinforced to the persisting S_D and to the rather differently persisting traces of S_1, S_2, S_3, S_4 and S_G.

Hull then goes on to explain why r_gs are evoked in such a way that they are not observed.

> It follows that on a repetition of this sequence there will be a tendency for S_D, together with the traces of S_1, S_2, S_3, and so on, to evoke R_G at the outset of the sequence and more or less continuously throughout it except insofar as there may be a conflict between R_G and the necessary instrumental movements of the sequence, such as R_1, R_2, R_3 and R_4. Presumably in any such situation the instrumental acts would dominate the conflicting position of the antedating generalized act, permitting the non-conflicting or fractional portion to persist in a covert form.

This fractional portion of the response he calls r_g. This r_g in its turn generates s_g, a characteristic proprioceptive stimulus. The animal produces such proprioceptive stimulation through its own responses. The fractional antedating goal reaction occurs as a part of a chain of stimuli and responses which are reinforced and the r_g and the s_g generated by it are like other stimuli and responses. They therefore obey the same rules as were outlined above for other stimuli and responses.

Now how is the introduction of this notion used by Hull (following the suggestion of Spence, Bergmann and Lippitt) to overcome the difficulties which the Kendler double-drive experiment offers his theory?

Spence, Bergmann and Lippitt say (1950, p. 549) that '. . . the stimulus cues in the water arm end box become, during training, conditioned to fractional anticipatory drinking acts (r_w) and, in turn, the proprioceptive cues (s_w) resulting from these anticipatory acts become conditioned to the response of entering the alley'. A similar conditioning of anticipatory eating acts will take place in the food alley. What this means is that the stimuli at the choice point, which can be called S_c, will, through a process of reward, come to evoke the fractional components of the goal responses of eating, r_{ge}, as well as the responses of turning right and left, R_R and R_L. r_{ge} will in turn produce s_{ge} which is again in its turn conditioned to produce R_R.

> During the test series [Spence, Bergmann and Lippitt go on to say (loc. cit.)], when only one drive is operative, the anticipatory act related to the goal for which the subject is motivated, will, because of the greater strength of the particular drive stimulus, Hull's principle of stimulus dynamism, be much stronger (more vigorous) than the other, and hence will produce stronger proprioceptive cues. Thus, if S is thirsty, proprioceptive cues from anticipatory drinking responses will be stronger than those from anticipatory eating. As these cues (s_w) will be conditioned to the response of entering the alley leading to water, they will tend to give this response the greater excitatory strength (stimulus dynamism).

However this argument breaks down and, carried to its proper conclusion, would predict an inability of the animal to select, Deutsch (1956). In the above quotations it is assumed that the fractional antedating goal responses both occur at the choice point simultaneously during training. If r_{ge} and r_{gd} occur, then s_{ge} and s_{gd}, the proprioceptive stimuli generated by them, must occur simultaneously also. Therefore, as both s_{ge} and s_{gd} are produced at the same time in whichever goal-box and by whatever goal substance the animal is rewarded, both s_{ge} and s_{gd} should be attached to both the response of turning right, R_R, and of turning left, R_L. They should both be attached to the alternative responses in the same way as the drive stimuli of hunger (S_{Dh}) and the drive stimuli of thirst (S_{Dt}) in Hull's own previous analysis.

Spence, Bergmann and Lippitt note themselves that during the process of training in the maze both drives (SD_h and S_{Dt}) will tend to be conditioned to evoke both r_{ge} and r_{gd}. But they say that owing to the past history of the animal which usually drinks when it is only thirsty and eats when it is only hungry, a learnt connection will arise only between S_{Dh} and r_{ge}, and S_{Dt} and r_{gd}. They further assume that the effects of this learning will be much greater than of the learning in the maze when the animals are both hungry and thirsty and an 'irrelevant' connection takes place. Whether this is the case or not, it still does not affect the crucial connections between s_{ge} and R_R and R_L (and similarly of s_{ge}), a connection which is only made in the maze and on which the animal's performance should depend. However, this suggestion does raise the interesting question whether drive discrimination has to be learnt or whether it is in some sense of the word innate. The only direct evidence on this matter comes from an experiment by Kendler (1949) and by Deutsch (1958) and is not completely conclusive. Kendler carried out an experiment on two groups of rats, one of which had been normally raised on food and water and the other of which had been given a milk-iron-copper-liquid diet after weaning. The training situation consisted of a T-maze with dog-checkers [small dog-biscuits] inserted in one box, whereas a milk and water solution was placed in the other. Water could not be used because the rats brought up on the milk mixture refused to drink water.

> During the test-trials [says Kendler], which consisted of one trial daily following deprivation of either food or the milk-water solution, the performance of the experimental and control groups were essentially similar. The percentage of appropriate responses in groups could be attributed to chance. Two S's in each group gave definite evidence, however, of having learned the appropriate habits.

These animals maintained their success for eight successive trials. However, the results of these two animals must be treated with caution. For an experiment to be convincing, there must be a significant proportion of success on the first test trial. An animal successful by chance on the first trial is already being given an opportunity to learn under circumstances of separate drive and separate reward. That is why the two animals which maintained their success on eight successive trials cannot be regarded as conclusive evidence against Spence's hypothesis that animals must learn to drive-discriminate. The results cannot be said to support Spence in any way either. Neither group, experimental or control, solved the problem and Kendler had already shown that a normal group of rats can solve this problem. Their failure in this instance is probably attributable to the fact that the milk and water solution presented to the animals in the maze is, after all, food as well as water, and the animals were therefore not presented with a clearcut choice.

However, even if a significant result had emerged on the first trial of the test series, it would not have been free of criticism. Kendler says: 'The motivational condition was controlled by permitting the animals an unlimited amount of milk-water solution and Purina dog Chow Checkers during a one hour feeding period occurring 21·5 hours prior to the experimental session the following day.' This would give the rats an opportunity of being separately reinforced when they were separately hungry or thirsty.

In Deutsch's experiment these difficulties were avoided. 21 rats were brought up on wet mash and trained in a T-maze which contained food on one side and water on the other. The animals were run when they were both hungry and thirsty. There was only one test trial. The animals were divided into two groups according to whether they had chosen the food or water side on the last day of training. Then they were left with either food or water in the cage so that they would have to choose correctly against the side which was preferred on the first trial of the previous day. Taking the results as a whole, the animals show a significant tendency to choose appropriately. However, because of the asymmetry of the correctness of the choices between the hungry and thirsty groups the result is not as convincing as it might be, though there are possible reasons for this. The difficulty is evident when it is stated that the thirsty group chose water correctly with only 2 of the animals going wrong. The other 8 animals chose correctly in spite of an established preference for the food path. However, in the case of the 'hungry' animals (some of which drank after taking the 'wrong' choice), only one out of the eleven animals chose correctly.

For further evidence about the innateness of object-choice under physiological deficit (this time, salt) the interested reader should consult Thompson and Porter (1953) and Epstein and Stellar (1955). The latter authors have demonstrated that the taste for salt and even the ability to estimate its deficit are not learnt.

A further variant of the drive discrimination experiment has been performed during the investigations of latent learning, of which one may instance that by Spence, Bergmann and Lippitt. Here animals merely come across food on one side of a T-maze and water on the other when they are neither hungry nor thirsty. They are induced to run the maze by giving them an opportunity to meet another rat at the end of their run. Now if this reward occurs at some distance from the locus of the food or water, the rats, when made either hungry or thirsty, tend to respond appropriately. (For a fuller discussion of this subject, see Deutsch 1956b).

Experiment I. To investigate whether the rat, hungry and thirsty and rewarded in separate places for food and water, can then select the appropriate place when it is left hungry (or thirsty) and satiated for water (or food). It is possible to replicate Kendler's double-drive experiment in the practical class with some modification. It is evident, however, that a statistically significant result cannot be obtained in one afternoon's work. However, the interest in work with animals (or indeed in any experiment) should not be exclusively in the result. Mere observation and familiarity with the animal brings a fresh insight and life into the problems of animal work, which cannot be appreciated by reading only. However, results are important and it is suggested that a score should be kept by the class so that individual experimenters can each contribute to a final tally.

The experiment to be carried out consists essentially of taking an animal which is both hungry and thirsty and rewarding it by allowing it to eat in one place and drink in another. The animal is then satiated for water, and then its choice of goal is noted on the next run.

Apparatus. A simple T-maze with goal-boxes which cannot be seen from the choice point. The floors of the goal-boxes should be covered one with cloth, and the other with paper. A small dish of water is placed in one goal-box and a small food receptacle is placed in the other.

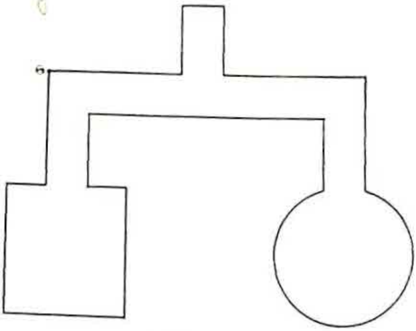

FIG. 7-1.

Subjects. The rat should not be younger than three months or older than six months. Below the lower age, deprivation has effects which are too severe, above the higher age the animal does not learn so well. The animals should be run about 1 hour before the usual feeding (or drinking) time. They should have been thoroughly tamed as pointed out above.

Method. The rat should be placed at the starting point by the experimenter, with its head pointing away from the choice point. This is customary because it is thought that in this way the experimenter is less able to impart an unconscious bias to the animal's choice by pointing it towards one of the alleys. The animal should be left to eat in the goal-box for five seconds and to drink for three. In this way it is unlikely to become satiated at the beginning of the experiment. After the animal has consumed the reward substances for the above times, it should be picked up and replaced at the start. Twenty trials in all should be run. After the twentieth trial, the animal should be allowed to drink its fill (preferably not in the maze; the starting box is a good place). It should then be replaced at the starting point. Its choices should be recorded for another ten trials. The use of blocks should be avoided throughout, unlike the conditions in Kendler's original experiments (cf. Deutsch and Anthony 1948).

Results. It is only the first choice of each animal which is really significant. The number of animals which can be run in any one period is to a large extent a matter of luck. Some animals respond with alacrity, others with torpidity and there are those which simply refuse to budge. However, it may be instructive to compare the recorded sequence of choices when the animal was acting under both drives with those executed when it was only acting under one, and to use the sequence of the animal's previous runs as a sample to test against the sequence executed when it was no longer thirsty. This will give some answer to the question: Given that an animal has learnt the two behaviour sequences, how well can it discriminate its drives? But it will not answer the other question Kendler set out to answer: Has the animal learnt which behaviour sequence led to the satisfaction of either one of the drives when both were present? Only the first trial provides a test of this, because after this test trial the animal may already be exhibiting learning under only one drive (which took place during the test trial).

Discussion. The experiment on double drive discrimination raises several and important issues. It is evident that the animal's abilities in this situation cannot be explained on the conditioning principles of Hull. Hull regards drive as a persistent stimulus (as Freud did a long time before him, for instance in *Instincts and their Vicissitudes* (1915), p. 118, Standard Ed. Vol. XIV, and in Section I part I of his *Project for a Scientific Psychology* (1895)). This stimulus for Hull becomes attached in learning to a response when their conjunction occurs close to the diminution of this stimulus. It is this removal of the stimulus which is rewarding or satisfying. Here again there is a close parallel between Hull and Freud. 'What does away with a need', says Freud (*Instincts and their Vicissitudes*), 'is satisfaction. This can be attained by an appropriate ("adequate") alteration of the internal source of stimulation.' This expression of Freud's 'principle of constancy' is very close to Hull's thinking. Unfortunately, these basic notions appear on closer examination to be without a great deal of foundation. Freud never worked them out closely in relation to the type of situation we are considering. But Hull's own elaboration does not seem adequate to explain even the very simple experiment we are considering. The notion that drives work like environmental stimuli in selecting responses is probably at fault.

Another way of considering the matter has been put forward by Deutsch (1953, 1956). In this theory, reinforcement is not due to diminution in drive. Stimuli are linked together in the order in which they occur, forming sequences. Some sequences may have the stimuli of water incorporated in them and some those of food. When the animal is thirsty, the parts of the sequence which correspond to 'water' are excited by dehydration, and the excitation spreads to the rest of the sequence. When a sequence is excited, the stimuli in it become attractive to the animal. That is, the animal begins to steer towards them. As the sequences are formed independent of the drive states and represent only the order of the incoming stimulation, two separate sequences build up, one with 'water' in it and the other with 'food'. When the 'water' sequence becomes excited on its own, only the stimuli leading to water are attractive. In this way the animal takes the path appropriate to its need.

EXPERIMENT II

Spontaneous alternation in the rat. In most experiments on rats in a T-maze, a reward is placed in one of the arms of the maze and the rat soon learns to which side of the maze to run in order to obtain the reward. The successive choices of the rat exhibit a statistical dependency. A run to the side which is rewarded raises the probability of a run to the same side on the next trial and lowers the probability of a run to the opposite side. Further, the greater the number of past runs to the side which contains the reward, the more likely is another run to this same side. Accordingly, if a rat is run in a maze where there is reward on both sides, we should expect one side, if there is no initial bias, to have a larger number of runs by chance. This chance majority we should then expect to determine the rest of its runs. Having run, say, three runs to the right and only one to the left, we should expect as more likely a repetition of the most frequent previous choice. However, this is not the case. Nor is it the case that the choices of the animal do not influence each other successively. They are not random. Instead we tend to observe a regular pattern of spontaneous alternation. Having chosen left, the animal is very likely to choose right. Tolman described this unexpected tendency as long ago as 1925.

> A simple T-maze was used, and it was arranged so that the animal could get back to the food box in identical fashion whether he chose the left or the right angle of the T. Either route met with success . . . [There was] a very pronounced tendency towards continuous and regular alternation left, right, left, right or right, left,

> right, left . . . It appeared, in short, that even where either side was equally 'satisfactory', there was in our rats a positive tendency left over toward variation of response.

This tendency to alternate without any training was again observed by Wingfield and Dennis (1934) and then further investigated by Dennis (1939).

This work was continued by Heathers (1940). He found, using a T-maze, that the likelihood of the repetition of a rewarded response went up the longer the time between the first and second choice. A repetition of a response was very improbable at 15 seconds between choices. He also tested at intervals of 30, 60 and 120 seconds and he found that the probability of an alternation decreased with increasing time interval. Such a finding has an important bearing on the problem of the lower efficiency of massed (i.e. closely spaced) trials and spaced (i.e. widely spaced) trials in learning. We should expect, according to the findings of Heathers, that there is less repetition of responses, whether they are correct or incorrect, when they are widely spaced in time than when they are closely spaced. This is indeed what was found by Ericksen (1939, 1941) who found that massed practice increases the variability of responses on successive trials.

What is the explanation of this curious phenomenon? Some of the investigators of this phenomenon (e.g. Zeaman and House 1951) have tried to explain it in terms of Hullian theory, or more explicitly in terms of his eighth postulate (the Miller-Mowrer hypothesis). This states that 'all responses leave behind in the physical structures involved in the evocation a state or substance which acts directly to inhibit the evocation of the activity in question' (1943, p. 7). This state or substance is called 'reactive inhibition'. It is supposed to vary directly with the amount of work involved in the response (though it is never made entirely clear whether this is 'work' or 'effort'). (A summary of the influence of work on subsequent behaviour is to be found in Solomon (1948), a review which should be approached critically.) Reactive inhibition is further supposed to dissipate with time (e.g. Heathers 1940 *supra*).)

Accordingly the occurrence of a response reduces the probability of its repetition. Further, it would be predicted that the probability of repetition is reduced by the number of times it has been executed and the amount of work expended in each response. Zeaman and House (1951) found that the first was the case. Animals which were forced to run down one of two alleys many times in succession had a heightened preference for the other when it was opened. However, Mowrer and Jones (1943) did not find support for the second part of the prediction. They did not find that animals would alternate more when the two levers, either of which they could press to obtain a reward, were more heavily weighted. However, they did find that extinction did proceed more quickly when the levers were heavier as would be predicted, but this finding has been shown to be artefactual by Trotter (1956).

There are other difficulties for this theory. Dennis (1939) found that rats did not alternate in their successive turns in a multiple square maze. They did not go right, left, right, left, but might turn left all the time. But they would alternate their successive choices at the same choice point. That is, having gone left on the previous trial at a certain choice point, they would tend to go right at that point on the next trial. Though this is not a clearcut objection because of the turns a rat has to make between one choice point and the next, it does suggest that a different factor is at work.

A more direct difficulty for the theory has been posed by Jackson (1941). He attempted to vary the amount of similarity in the two responses to be made at the choice point. He used a Y-maze with an angle in the Y which could be varied. The amount of alternation obtained did not correlate with the size of this angle. However, the most clearcut demonstration that the reactive inhibition of a response is not the operative factor has come from Montgomery (1952). This worker trained rats in an X-maze with equal rewards on the ends of one of the diagonals. He thus had a choice of starting point. If rats alternate their responses they should run toward the same goal if they are started from two different starting points in succession. On the other hand if they alternate on a principle of stimulus or goal alternation they should alternate between the two sides in spite of having to execute two similar turning movements in a row. Montgomery found that the results obtained in this situation did show stimulus or goal alternation. What seemed to matter was the direction of the previous run rather than the previous turn. Glanzer (1953b) performed substantially the same experiment with a similar result.

Glanzer (1953) has suggested that the phenomena we have been considering can be explained in terms of stimulus alternation, rather than in terms of response alternation. He assumes that if a stimulus at the choice point has been effective in eliciting a response, its effectiveness will be diminished and only gradually return to normal in its capacity to elicit a response. Though this suggestion would explain the phenomena so far considered, Glanzer seems somewhat hazy by what he means by a stimulus. He seems mostly to mean 'stimulus' in the accepted sense, that is, some afferent process generated by receptors and indirectly by some physical energy falling on them. However, in some deductions from his theory he uses the word 'stimulus' for parts of the animal's environment which are not at that moment impinging on the animal's receptors. For instance, he says (p. 264) 'The author found that when rats were detained in the end box of the alternative chosen in the first run in a T-maze, 96 per cent of the group showed spontaneous alternation on the following trial . . . When they were not detained, spontaneous alternation varied between 72 and 88 per cent.' In a further paper (Glanzer 1958), he claims that the tendency of a rat to run to the goal-box other than the one in which it has been placed, constitutes a support for his generalization of 'stimulus satiation'. He does not attempt to justify this extension of the word stimulus. If an animal has been placed in a goal-box which is not visible from the choice point, then chooses to run to the other goal-box when it is again placed at the choice point, this can hardly be attributed to the lessened efficiency of the stimuli in the first goal-box to elicit responses. This goal-box simply does not stimulate at the choice point and it is difficult to see what Glanzer can mean. Perhaps he means 'potential stimuli' but of course potential stimuli are not actual stimuli to an animal and can hardly be physically effective. The theory does not explain how an alteration in the effectiveness of stimuli which are anyhow absent can affect the animal at the choice point.

Another suggestion which has been made to explain spontaneous alternation is that involving exploration or curiosity. This has been put forward by Montgomery himself (1952) and Berlyne (1951). To some this may seem to be a case of *obscura per obscuriora*, but the theory does have some points of plausibility. It has been shown (Berlyne 1955; Dember 1956) that rats will approach novel stimuli. If a rat has turned one way in the T-maze, it is more likely on the next occasion to turn the other, because this is the way which is now more novel. But such a suggestion seems unlikely for the following reason. Sutherland (1957) and Glanzer (1958) have found that rats, after they have been manually placed in one goal-box, will tend to run to the other when given the choice. Berlyne and Slater (1957) have found that rats will not learn to run to a goal-box in a T-maze where they are presented with novel stimuli, even though they approach these when they come across them in the goal-box. It appears that a rat will not choose a path only because there is a novel stimulus at the end of it, where it cannot be sensed from the choice point. However, in Sutherland's and in Glanzer's experiment, both maze arms visible from the choice point were equally novel. It was only one of the goal-boxes which the animals could not see which were more novel, and it was to these that the animals ran. If animals do not run to novel situations when they are out of sight (or not impinging on the sensorium), Sutherland's or Glanzer's animals could not have been alternating because of curiosity or explorativeness. As a direct test of this assertion, Deutsch and Clarkson (unpublished study) have found that though rats show large differences in their tendency to alternate and to approach novel stimuli, these differences did not correlate at all in the group of 24 animals they used. This makes it highly unlikely that curiosity causes alternation or indeed that there is any common cause in the production of these two phenomena.

A theory which can account for the phenomena of spontaneous alternation within a larger framework has been put forward by Deutsch (1953, 1956). This theory was able to predict goal alternation in advance 1953 and thus Sutherland's (1957) and Glanzer's (1958) experiments provide a test of it. We should on this theory predict that *if an animal is equally rewarded and trained on two sides of a T-maze, where goal-boxes are out of sight at the choice point, then if it is directly placed in one of the two goal-boxes and rewarded there, it should tend to run to the other when it is placed again at the entrance of the maze and given a free choice. This prediction is confirmed by the two experiments quoted above.*

Experiment. To investigate the response dependency between a rat's successive choices of two equally rewarded alternatives and to study the factors which influence this dependency. It is possible to replicate some of the spontaneous alternation studies with quite good results. The first part of the experiment consists in training an animal to run a T-maze with both goal-boxes containing a reward. The fact of regular alternation can be verified in this way. The second part consists in leaving the animal four minutes between trials first outside the maze and then confined in the goal-box it has chosen. The third part of the experiment is to place the animals in one of the end boxes directly, rewarding them there, and to observe their behaviour on the following trial. Except for the initial training period, the parts of the experiment ought to be done in a different order for each rat.

Apparatus. A simple T-maze, as in the previous experiment. The two goal-boxes should again be differentiated by using different kinds of floor covering. Two small dishes, again different, should be placed in the two goal-boxes. A solid wooden block, the width of the alley, should be to hand. This is to confine an animal in a goal-box. The use of this is preferable to that of one way swing doors, as most animals are rather shy of these.

Subjects. These should be thoroughly tamed rats from three to six months old. They should be run about 1 hour before usual feeding time, having been accustomed to this schedule for some weeks. Otherwise they might be too hungry for optimum alternation. If they have not been previously accustomed to the schedule, 8 to 12 hours before feeding is desirable.

Method. The rats should be placed in the maze by the method described in Experiment I. They should be allowed to eat for five seconds in each goal-box, When it is necessary to detain them in a goal-box, the wooden block should be placed in the entrance of the goal-box. The rat should be given frequent opportunities to drink throughout the experiment. Otherwise they may not eat owing to excessive thirst. If it is possible, wet mash ought to be given as a reward in the goal-boxes and then drinking need not interfere with the conduct of the experiment. Twenty trials should be given in the initial training, and ten in each subsequent condition.

Results. The results may be assessed by counting the numbers of RL and LR combinations as against the number of LL and RR combinations (R = right, L = left). In a strictly random sequence, the percentage should be 50 per cent. Percentages may be compared between the different conditions. The significance of the results may be assessed by employing standard statistical techniques.

Discussion. The spontaneous alternation experiment is one of the most convincing in demonstrating the remarkable regularities in behaviour and the effects of sequential response dependency. Similar effects are of importance in human experimentation. Response dependencies have been demonstrated even in the remote field of threshold measurement by Howarth and Bulmer (1956). However, in this case it is not clear whether it is the response itself which generates the dependency or whether it is the stimulus generated by the response which is the causal factor. In the rat's case, it appears to be the stimulus or goal factors which are responsible.

In this experiment, even more clearly than in the last, it is shown in the Sutherland-Glanzer situation that the animal's choice can be influenced by the goals of its activity. These goals control the animal's activity in a way which cannot be described in stimulus-response terms. For when the animal is actually making the choice, it seems influenced not only by what it senses or perceives at the time but by an expectation of the consequences of its actions. This way of talking is of course no explanation but it does seem to give a closer description of the animals' behaviour than more cumbersome, circuitous and respectable periphrases. Psychologists have reacted in two ways to this embarrassing behaviour on the part of the rat. The first way has been that of Tolman. The description in terms of expectancy has been treated as an explanation. The problem is solved by attaching the comforting labels of everyday speech to the phenomena. The other way has been like that of Hull. This is to derive the phenomena against all plausibility and even logic (see Deutsch 1956b) from supposedly more basic phenomena such as the principles of conditioning. However, such phenomena are so widespread and so striking that they deserve attention in their own right.

As has been mentioned above, Deutsch (1953, 1956) has put forward a theory, according to which stimuli are formed into sequences depending on the order in which they occur. When excitation is introduced into this sequence, from one end, for instance, the stimuli in this sequence will pass it on from one to the next till the whole sequence is excited. When a sequence is excited, the stimuli in it become attractive. However, if a stimulus in the sequence actually occurs and is sensed by the animal, excitation ceases to be transmitted by it to the other stimuli which were having it passed on through it. This transmission gradually resumes as time passes. In this way the phenomena of spontaneous alternation may be understood. If there are two sequences and the attractiveness of a stimulus is proportionate to the amount of excitation it is receiving, then if one sequence has been sensed it will transmit less excitation and so be less attractive for the moment. Similarly, if the part of the sequence nearer the source of excitation is sensed without all the sequence having also been sensed, no excitation will be passing momentarily through the stimuli nearer the source, so that the further stimuli in this sequence at the choice point will themselves not be attractive. Though this argument is rather loosely put for the sake of brevity, it led (Deutsch 1953) to the prediction of the Sutherland-Glanzer phenomenon. Exposing an animal to stimuli anywhere in the sequence should make the members of the sequence further from the source of excitation less likely to be chosen.

EXPERIMENT III

Latent extinction. Latent extinction is so-called by analogy with latent learning. In a latent learning experiment an animal is shown a reward for which it is not motivated during its meanderings in a maze. Though its performance at the time does not seem to be affected by this reward, such learning has in some senses taken place. The animal changes its performance as soon as the reward becomes relevant to its needs, or at least does so in some situations. In other latent learning experiments, the animal is only shown a complete environment such as a maze with no reward it in. Though at the time its performance gives no hint of it, as soon as a reward relevant to its needs has been found by the animal, a change which can only be attributable to past learning comes over its behaviour in the further series of trials. In these cases, the correct instrumental sequence of responses suddenly occurs when a reward or a need for it occurs. In latent extinction, the correct instrumental sequence of responses disappears when the reward disappears. But the instrumental responses disappear without having been performed. For instance, if a rat has been placed in a goal-box (previously baited with reward when it has run to it) and found it empty, it will then show a reluctance to run to this goal-box and prefer another when it is given the choice. Thus latent extinction is really the extinction of an instrumental act without its performance.

Hull of course has no difficulty in interpreting the data on ordinary extinction on his eighth postulate (concerning reactive inhibition). All responses when made leave behind them a state which opposes their repetition. Usually, when a response is rewarded, this tendency not to repeat the response is more than compensated for by the reward. When reward fails to occur repeatedly, then the state opposing the repetition of the response builds up to such an extent that the response is no longer repeated. However plausible such a view might be when a response fails to appear after actually having been elicited, it cannot, as it stands, explain the case where the response fails to appear where it has not occurred.

Cases of this kind have now been reported quite frequently in the literature. (For a summary, see Moltz 1957.) For instance, Seward and Levy (1949) have reported an experiment of this kind. In this, two groups of rats, E and C, ran down an elevated pathway from a starting platform to a goal platform for a food reward. After some training, each rat in Group E was placed on the goal platform with no food on it. The C group was identically treated except that it was placed on a strange platform. 'Five such treatments were given . . . On the following day all rats were given extinction trials to a criterion of two successive refusals to leave platform A (the starting platform) within three minutes. Group E reached the criterion in a mean of 3·12 trials, Group C in 8·25; the difference was significant at the ·01 level.' The experience of the empty reward platform seems to have reduced the animals' tendency to leave the starting platform.

Another study of the same type has been performed by Deese (1951). In his experiment, Deese rewarded rats on one side of a U-maze by giving them food. After the rats had been trained, he placed those in the experimental group in the empty goal-box for four one minute periods. The control group was given no such training. The experimental animals, which were afterwards run to the goal-box, did so a smaller number of times than the control group.

Though the above data seem directly to contradict the Hullian viewpoint, Moltz has recently attempted an explanation of latent extinction by an extension of Hull's theory. This he attempts to do in terms of antedating goal reactions. Hull suggested that a part of the stimulation in a goal could be displaced and occur in parts of the stimulus-response sequence remote from it. He assumed that a stimulus, when it was triggered off by an external event, would persist longer than this external event and would take some time to die down. This persisting trace of the stimulus, Hull assumed, could be linked to responses occurring much later in time. The responses which had in this way been conditioned to the trace stimuli could also be evoked by the stimuli as soon as they occurred before they became traces. However, these responses would be made more weakly and would therefore be swamped except for certain slight responses which would not conflict with the stronger responses. As these responses, the important ones of which were fractions of the responses in the goal, occurred before the goal and were made prematurely, they were called antedating goal reactions. These antedating goal reactions (or r_gs) are held to generate proprioceptive stimulation (or s_g). As the r_gs will occur regularly before the animal is rewarded in the goal-box, these r_g-s_g combinations will become a part of the stimulus-response chain leading to the goal, paralleling the main chain of stimulus-response (S-R) linkages.

$$\begin{tikzcd}
S_1 \arrow[r] \arrow[d] & R_1 & & S_2 \arrow[r] \arrow[d] & R_2 \\
r_g \arrow[r] & s_g \arrow[u] & & r_g \arrow[r] & s_g \arrow[u]
\end{tikzcd}$$

FIG. 7-2.

In the above diagram, if S_1 is a stimulus and R_1 is the response evoked by it, S_1 evokes r_g as well as R_1, r_g in its turn produces s_g. s_g in its turn evokes R_1. In this way, R_1 is evoked jointly by S_1 and s_g.

Moltz suggests that latent extinction occurs when an animal is placed in an empty goal-box where it has previously found reward because antedating goal reactions occur repeatedly in the goal-box. As they are not followed by reward they suffer extinction through the accumulation of I_R or reactive inhibition. As these antedating goal reactions occur as ingredients of the stimulus-response chain leading to the goal, they will not be evoked at a reduced strength, their characteristic proprioceptive stimuli will also undergo diminution. Therefore a part of the stimulation which normally evokes the responses leading towards the goal will be missing and partial extinction will have taken place.

That this is the case is made clear by referring to the diagram above. If the r_g-s_g component is removed, only S_1 will be acting to evoke R_1 and S_2 to evoke R_2. Consequently we should expect R_1 and R_3 to occur with diminished vigour.

However, Deutsch and Clarkson (1959) conducted a test between this theory and that of Deutsch (1953, 1956). On Deutsch's theory, extinction does not depend on response evocation at all. On this theory, a sequence is formed of receptoral mechanisms corresponding to the order of their stimulation by the environment. Excitation is passed on down this sequence from one member to the next. When a member is excited, then the corresponding feature in the environment becomes attractive. Failure of one stimulus to follow another, which precedes it in the sequence, brings about diminution of the excitation flowing from one member to the other. Hence all the members on the side of the break further from the source of excitation will undergo a lessening of attractiveness. For instance, if food is left out of a goal-box and this is shown to a rat which has been running to the goal-box for this reward, then the cues at the beginning of the maze should be affected by this experience and should not be approached so readily.

The test between the Hullian view and the view stated above was as follows. A maze was made with two alternative routes of the same length leading to a common goal-box. The rats used in this experiment were given a free choice of path and were rewarded by food in the goal-box whichever choice they made. In extinction, two different procedures were used. The first consisted of leaving the goal-box empty. According to the Hullian view, the antedating goal reaction (r_g) should be aroused in the goal-box and suffer extinction as it is not followed by reward. This extinction should affect the stimulus-response sequences corresponding to the two routes equally. The r_g-s_g components of the habits should be knocked out to the same degree in both paths. Therefore the prediction which follows is made from Hull's theory without Moltz's addition, as the factors which Moltz postulates should be equal for both parts. On Hull's theory, reactive inhibition will lead to the extinction of a response which has been performed unrewarded. Hence the turn which has just been made previous to the entry into the empty goal-box should be extinguished, whereas the response of going via the alternative route should be unaffected. Hence the animals should on the subsequent trial show an increased tendency to like the alternative route. On the other hand, Deutsch's theory would say that both sequences had been equally interrupted by the rats' experience in the goal-box and accordingly that alternation should remain at the pre-test level.

In the second condition, extinction was produced by blocking the paths leading to the goal-box just before the entrance to it. Here both theories would predict a switch to the alternative path. The group was run to demonstrate that the animals had indeed distinguished the two alternative paths. The other reason was to obtain some kind of baseline for a prediction in the other condition on Hull's theory, because the two conditions (goal-box empty, path blocked) are very similar from the Hullian view-point. They are however quite different on Deutsch's theory.

On the test run with the goal-box empty, nine out of nineteen animals alternated, 8·8 alternations per day being the average figure for the previous days. However, with the path blocked, there were 17 alternations out of a possible nineteen. The result accords well with the prediction made from Deutsch's theory. On the other hand, if Hullian theory cannot explain the present result, it is unlikely that it does correctly explain other latent extinction phenomena, involving as they do a capacity on the part of the animals similar to that tested in this experiment.

Experiment. To investigate the effects on a rat's subsequent behaviour of discontinuing to reward it in a goal-box, and to determine whether it is necessary for responses to be evoked before they can be extinguished. This is to test the efficacy of a latent extinction procedure and compare it with an ordinary extinction procedure. A T-maze is used with a reward in both goal-boxes. The animals are trained in the same way as in the alternation experiment, being given a free choice of the two boxes. However, after every second free trial after the tenth, when the animal should have learnt about the identity of the goal-boxes, it should be taken and inserted in one of the goal-boxes and rewarded, being kept there for 20 seconds (the usual feeding time in a box should be five seconds as in the alternation experiment). The two goal-boxes should be chosen by the experimenter in random order when he is inserting the animals. The order of the animals' choice should be recorded, both after the animal has made a free choice and after it has been inserted in a goal-box by the experimenter. After the twenty training runs, when the animal is familiar with the maze and with the experimenter's procedure, the test trials can begin. These consist of giving the animal experience of an empty goal-box when it has run to it itself and when it has been placed there by the experimenter. The test trials should be continued until the animal chooses the previously empty goal-box itself. Six training runs should be given between the tests and the whole procedure continued till the animal shows signs of satiation. [Make sure the animal is not thirsty. When it is thirsty it behaves as if it is not hungry (Verplanck and Hayes 1953).] The training runs after testing has started should continue to be interpolated with rewarded insertion in the goal-box.

Apparatus and subjects. The same as in Experiment II (or spontaneous alternation). It would be advantageous actually to use the same animals as in the previous experiment because of their familiarity with the situation.

Results. Because of the difference in procedure on the previous trial, the results from the latent extinction condition and the ordinary extinction trial are not strictly speaking comparable. However, the effects of latent extinction can be compared with those of ordinary extinction by taking two different baselines. The first is the behaviour of the animal on the trials after it has been placed in the goal-box during the test series. This can be compared with the behaviour after it has been placed in the goal-box empty. The other baseline is the behaviour of the animal after it has run to the goal-box when it is full. This can be compared with the behaviour of the animal which has run to the goal-box and found it empty. If the two baseline figures show agreement, then the two conditions can be directly compared.

A subsidiary experiment on extinction. More dramatic results can be obtained by training the animal as in the spontaneous extinction experiment for twenty trials and then blocking one of the alleys near the goal. The effect of this may be compared with placing the animal in the opposite alley with blocks screening it from the choice point and the goal-box (a latent extinction condition).

The more pronounced effects in this condition in causing the animal to switch to the other goal-box, would be ascribed by Hullians to the smaller amount of secondary reinforcement in the alley than in the goal-box, the box being more strongly associated with food. On the other hand, it might be said that there is less confusion or generalization between the alleys than between the goal-boxes, because a strong common cue occurs in both boxes. Design an experiment for yourself, testing your own or the above hypothesis to account for the difference.

REFERENCES

BERLYNE, D. E. (1951). 'Attention, perception and behaviour theory'. *Psychol. Rev.*, **58**, 137.

BERLYNE, D. E. (1955). 'The arousal and satiation of perceptual curiosity in the rat'. *J. comp. physiol. Psychol.*, **48**, 238.

BERLYNE, D. E., and SLATER J. (1957). 'Perceptual curiosity, exploratory behaviour and maze learning'. *J. comp. physiol.*, **50**, 228.

BOLLES, R., and PETRINOVICH, L. (1954). 'A technique for obtaining rapid drive discrimination in the rat'. *J. comp. physiol. Psychol.*, **47**, 378.

DEESE, J. (1951). 'The extinction of a discrimination without performance of a choice response'. *J. comp. physiol. Psychol.*, **44**, 362.

DEMBER, W. N. (1956). 'Response by the rat to environmental change.' *J. comp. physiol. Psychol.*, **49**, 93.

DENNIS, WAYNE (1939). 'Spontaneous alternation in rats as an indication of the persistence of stimulus effects'. *J. comp. Psychol.*, **28**, 305.

DEUTSCH, J. A. (1953). 'A new type of behaviour theory'. *Brit. J. Psychol.*, **44**, 304.

DEUTSCH, J. A. (1956). 'A theory of insight, reasoning and latent learning'. *Brit. J. Psychol.*, **47**, 115.

DEUTSCH, J. A. (1956b) 'The inadequacy of the Hullian derivation of reasoning and latent learning'. *Psychol. Rev.*, **63**, 389.

DEUTSCH, J. A. (1958). 'Double drive learning in rats without previous selective reinforcement'. *Quart. J. exp. Psychol.*, **10**, 207.

DEUTSCH, J. A. (1959). 'The Hull-Leeper drive discrimination situation – a control experiment'. *Quart. J. exp. Psychol.*, **11**, 155.

DEUTSCH, J. A., and ANTHONY, W. (1958). 'Blocking the incorrect alley in a two-drive learning situation'. *Quart. J. exp. Psychol.*, **10**, 22.

DEUTSCH, J. A., and CLARKSON, J. K. (1959). 'Reasoning in the hooded rat'. *Quart. J. exp. Psychol.* (in press).

DEUTSCH, J. A., and CLARKSON, J. K. (1959). 'A test of the neo-behaviouristic theory of extinction'. *Quart. J. exp. Psychol.* (in press).

EPSTEIN, A. N., and STELLAR, E. (1955). 'The control of salt preference in the adrenalectomised rat'. *J. comp. physiol. Psychol.*, **48**, 167.

ERICKSEN, S. C. (1939). 'A second study of the relative of a cerebral lesion upon learning, retention and transfer'. *Psychol. Bull.*, **36**, 634.

ERICKSEN, S. C. (1941). 'The effect of distributed and massed practice on the nature of errors made by normal and operated rats'. *J. comp. Psychol.*, **31**, 57.

FARRER, E. J., and GRIFFITH, J. Q., JR. (Eds.) (1949). *The Rat in Laboratory Investigations*, revised edition. Philadelphia: Lippincott.

FREUD, S. (1905). *Three Essays on the Theory of Sexuality*. London, 1949, Standard Edition, **7**, 125.

GLANZER, M. (1953). 'Stimulation satiation: an explanation of spontaneous alternation and related phenomena'. *Psychol. Rev.*, **60**, 257.

GLANZER, M. (1958). 'Stimulus satiation in situations without choice'. *J. comp. physiol. Psychol.*, **51**, 332.

HEATHERS, G. L. (1940). 'The avoidance of repetition of a maze reaction in the rat as a function of the time interval between trials'. *J. Psychol.*, **10**, 359.

HOWARTH, C. I., and BULMER, M. G. (1956). 'Non-random sequences in visual threshold experiments'. *Quart. J. exp. Psychol.*, **8**, 163.

HULL, C. L. (1933). 'Differential habituation to internal stimuli in the albino rat'. *J. comp. Psychol.*, **16**, 255.

HULL, C. L. (1943). *Principles of Behaviour*. New York: Appleton-Century.

HULL, C. L. (1952). *A Behaviour System*. New Haven: Yale University Press.

JACKSON, M. M. (1941). 'Reactive tendencies of the white rat in running and jumping situations'. *J. comp. Psychol.*, **31**, 255.

KEELER, C. E., and KING, H. D. (1942). 'Multiple effects of coat colour genes in the Norway rat, with special reference to temperament and domestication'. *J. comp. Psychol.*, **34**, 241.

KENDLER, H. H. (1946). 'The influence of simultaneous hunger and thirst drives upon the learning of two opposed spatial responses of the white rat'. *J. exp. Psychol.*, **36**, 212.

KENDLER, H. H. (1949). 'An experimental examination of the non-selective principle of association of drive stimuli'. *Amer. J. Psychol.*, **62**, 382.

KIVY, P. N., EARL, R. W., and WALKER, E. L. (1956). 'Stimulus context and satiation'. *J. comp. physiol. Psychol.*, **49**, 90.

LEEPER, R. (1935). 'The role of motivation in learning: a study of the phenomenon of different motivational control of the utilization of habits'. *J. genet. Psychol.*, **46**, 3.

MOLTZ, H. (1957). 'Latent extinction and the fractional anticipatory response mechanism'. *Psychol. Rev.*, **64**, 229.

MONTGOMERY, K. C. (1952). 'A test of two explanations of spontaneous alternation'. *J. comp. physiol. Psychol.*, **45**, 287.

MONTGOMERY, K. C. (1952). 'Exploratory behaviour and its relaxation to spontaneous alternation in a series of maze exposures'. *J. comp. physiol. Psychol.*, **45**, 50.

MOWRER, O. H., and JONES, H. M. (1943). 'Extinction and behaviour variability as functions of effortfulness of task'. *J. exp. Psychol.*, **33**, 369.

MUNN, N. L. (1950). *Handbook of Psychological Research on the Rat*. Boston: Houghton-Mifflin.

SEWARD, J. P., and LEVY, N. (1949). 'Sign learning as a factor in extinction'. *J. exp. Psychol.*, **39**, 660.

SMALL, W. S. (1900). 'An experimental study of the mental processes of the rat'. *Amer. J. Psychol.*, **11**, 133.

SOLOMON, R. L. (1948). 'The influence of work on behaviour'. *Psychol. Bull.*, **45**, 1.

SPENCE, K. W., BERGMANN, G., and LIPPITT, R. (1950). 'A study of simple learning under irrelevant motivational conditions'. *J. exp. Psychol.*, **40**, 539.

SUTHERLAND, N. S. (1957). 'Spontaneous alternation and stimulus avoidance'. *J. comp. physiol. Psychol.*, **50**, 358.

THOMPSON, C. W., and PORTER, P. B. (1953). 'Need reduction and primary reinforcement: maze learning by sodium-deprived rats for a subthreshold saline reward'. *J. comp. physiol. Psychol.*, **46**, 281.

TOLMAN, E. C. (1925). 'Purpose and cognition: the determiners of animal learning'. *Psychol. Rev.*, **82**, 285.

TOLMAN, E. C. (1945). 'A stimulus-expectance Need-cathexis Psychology'. *Science*, **101**, 160.

TROTTER, J. R. (1956). 'The problems of bar-pressing behaviour and the problem of inhibition'. *Quart. J. exp. Psychol.*, **8**, 97.

The UFAW Handbook on the Care and Management of Laboratory Animals, (1947); ed. Worder, A. N. London: Baillière, Tindall & Cox.

VERPLANCK, W. S., and HAYES, J. R. (1953). 'Eating and drinking as a function of maintenance schedule'. *J. comp. physiol. Psychol.*, **46**, 327.

WINGFIELD, R. C., and DENNIS, W. (1934). 'The dependancy of the rat's choice of pathways upon the length of the daily trial series'. *J. comp. Psychol.*, **18**, 135.

ZEAMAN, D., and HOUSE, B. J. (1951). 'The growth and decay of reactive inhibition as measured by alternation behaviour'. *J. exp. Psychol.*, **41**, 177.

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